The SCIENTIFIC MONTHLY

JAN. '48

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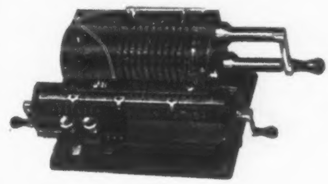

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Contents

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THE SCIENTIFIC MONTHLY

JANUARY 1948

EDMUND WARE SINNOTT

PRESIDENT OF THE A.A.A.S. FOR 1948

ALBERT F. BLAKESLEE

Genetics Experiment Station, Smith College; President, A.A.A.S., 1940.

SOMEWHAT over thirty-two years ago I was asked to recommend my successor for the position of professor of botany and genetics in what was then the Connecticut Agricultural College at Storrs. I recommended Edmund Sinnott. In so doing I told President Beach that I could appropriately use the words of John the Baptist in prophesying that "He it is, who coming after me is preferred before me, whose shoe's latchet I am not worthy to unloose." I trust John had an equal satisfaction in seeing his prophecy come true. In my own case the prophecy was not a wild gamble since, as an instructor in one of Professor Thaxter's classes in cryptogamic botany in 1907, I had known Sinnott as a brilliant young student and had followed his subsequent activities with a teacher's interest.

A biographer often looks to the ancestry of his subject and the surroundings to which he has been exposed for an explanation of achievement. Any close correlation is usually difficult to discover, but in the present case it seems safe to conclude that fate dealt a favorable hand of genetic cards and that early environmental influences, which are sometimes difficult to separate from heredity, also had a beneficent influence on the resultant product.

The father of Edmund Ware Sinnott was Charles P. Sinnott, whose ancestors came from Maine; Charles Sinnott's paternal grandfather was Irish, his grandmother French. The rest of the ancestry appears to be pure Yankee. Sinnott's mother's people (the Smiths) came from the Connecticut Valley and were descended from the Reverend Henry Smith, first minister of Wethersfield, Connecticut. His father graduated from the Bridgewater (Massachusetts) State Normal School and from Harvard. He taught various sciences, especially geology and geography, in a number of normal schools, but the longest period of teaching (1897–1936) was at Bridgewater.

Edmund Sinnott was born at Cambridge, Massachusetts, February 5, 1888. The family soon moved to Milwaukee, Wisconsin, and lived there until 1897, when they returned to Massachusetts. Here he was educated in the public schools of Bridgewater and then at Harvard, taking an A.B. there in 1908 and a Ph.D. in 1913. He was always interested in natural history even in his younger days. After he came under the influence at Harvard of Jeffrey, Fernald, Thaxter, Parker, and Castle, he definitely determined to be a biologist and was headed at first for zoology. Later, however, he became particularly interested in plant morphology under Jeffrey, and went on to graduate

ART BY E. W. SINNOTT

THE CONGREGATIONAL CHURCH AT FARMINGTON, CONNECTICUT, AND THE HEAD OF CLARA SINNOTT.

work with him. In 1910–11, in company with A. J. Eames (now of Cornell), he spent a year in Australasia as a Sheldon Fellow, collecting plant material, particularly of the conifers. This was in the good old days before World War I, when travel was cheap and uncomplicated. This experience in a region where the plant life was so entirely different from our own was very stimulating to him.

Under Jeffrey's guidance he was brought up in the strict tradition of comparative anatomy, the major goal of which was a reconstruction of the phylogenetic tree of the plant kingdom. His dissertation was on the reproductive structures of the Podocarpineae. He had published a number of papers before this, however, all on the comparative anatomy of the higher plants, for one of which he received the Bowdoin Medal from Harvard in 1910. For two years after the doctorate he worked at the Bussey Institution with I. W. Bailey. The two had set themselves the grandiose goal of going through the angiosperms and working out the phylogeny of all their families on the basis of structure; they published a number of papers together in this field. There was no opportunity to complete this plan, and in 1915 Sinnott left to become professor of botany and genetics at Storrs. Here his interests turned from the purely descriptive and comparative aspects of plant form to the developmental factors—genetic and environmental—that determine it. He believed that the cucurbits provide in their fruits excellent material for such a study and while at Storrs he investigated the inheritance of shape and size, the cellular basis of these characters, and the effects upon them of various external factors.

He has continued to use the cucurbits as the main object of his personal investigations, a recent phase of which has been a study of how cellular behavior determines the size and form of fruits of normal and polyploid plants. He has, however, employed other plants, especially in cooperative studies of the broader problems of differentiation and growth. Thus, he and I collaborated in studies of the genetics of *Datura* and worked out the anatomical correlates of the various chromosomal types. Greatly to our surprise, Sinnott was able to recognize these types by differences in anatomical structure seen in freehand sections of flower stalks with apparently the same ease with which we could recognize them by external appearance.

In 1928 he went to Columbia as professor of botany and chairman of the department at Barnard College; he was also on the graduate faculty of the University. He remained for twelve years, the last year leaving Barnard and being transferred entirely to Columbia. Dr. L. C. Dunn and Sinnott went to the University from Storrs the same year and there continued their earlier association and cooperation. While at Columbia Dr. Sinnott served for three years as president of the Torrey Botanical Club and still serves on the Boards of the New York Botanical Garden and the Boyce Thompson Institute for Plant Research.

EDMUND WARE SINNOTT

PRESIDENT, AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, 1948.

In 1940 he went to Yale as Sterling professor of botany, chairman of the department, and director of the Marsh Botanical Garden. This was when some in Yale held the old-time belief that botany consists mainly in identifying and naming wild flowers, and doubted the propriety of developing the subject as a major university department. Sinnott, however, put new life and meaning into the Department of Botany and in 1945 was chosen director of the reorganized Sheffield Scientific School. The Sheffield centennial, celebrated this fall in New Haven, was a significant scientific event. Yale University has strongly supported Sinnott's work, and he has been able to gather about him an active and enthusiastic group of staff members and students, some of whom have come to his department in preference to financially better offers elsewhere. One cannot visit the Yale Department of

Botany at the present time without sensing the fine spirit of joyous endeavor and happy comradeship under Sinnott's leadership.

In 1916 Sinnott married Mabel Haskell Shaw. They have three children: Edmund Ware, Jr., Harvard, 1938; Mildred Shaw, Swarthmore, 1941; and Clara Richardson, Connecticut College, 1945. During the summer the Sinnotts live in a delightful old farmhouse at Woodbury, Connecticut, with adequate land for the growth of squashes, where he carries on most of his experimental work.

A thing that surprises one regarding the subject of our sketch is his versatility—his excellence in so many characteristics. That he is a good teacher is attested by those who have had the privilege of being students in his classes. It is also shown by the successful textbooks he has written: *Botany: Principles and Problems,* now in its fourth edition; and *Principles of Genetics* (with L. C. Dunn), now in its third edition. His standing and contributions as a plant scientist have brought about his election to the leading honorary societies in this country to which he has been eligible: Phi Beta Kappa; Sigma Xi; National Academy of Sciences; American Philosophical Society; American Academy of Arts and Sciences. For the same reasons he has been elected president of the following societies: Botanical Society of America; Torrey Botanical Club; American Society of Naturalists; Society for Study of Development and Growth; chairman of Section G (Botany) of the American Association for the Advancement of Science, and now president of our Association. He is a facile writer and a forceful and fluent speaker.

Good judgment in problems of scientific organization and tact in human relations are responsible for his being much sought after on committees; for administrative positions, such as trustee of *Biological Abstracts,* New York Botanical Garden, Boyce Thompson Institute for Plant Research, Associated Universities, and membership on the National Research Council and its Committee on Growth, which helps in the research problems of the American Cancer Society; and for advice in other capacities. He has not shunned civic responsibility in scientific affairs when the labor much exceeded the honor involved. Thus, he served several years as editor of the *American Journal of Botany* and was an effective member of the committee that reorganized the *Journal.*

Many of the illustrations in his textbooks were made by Sinnott himself, but I did not realize the extent of his artistic ability until I saw over the mantel in his home a painting he had made of the Congregational Church at Farmington, Connecticut. Early New England meeting-house and church architecture is his hobby, and he has visited and photographed most of these buildings erected in the eighteenth and early nineteenth centuries. His art expression is not confined to oil painting. He has also done some creditable pieces of sculpture. One of these, of his younger daughter, Clara, is shown in the accompanying photograph, below his painting of the Farmington church.

That the reader may realize there are some limitations to the abilities of the subject of this sketch, it must be stated there is good evidence that he never aquired proficiency in music either as a performer or as a composer, that he never was interested in writing poetry, and that he never excelled in athletics.

Sinnott has been an inspiring leader among the students and associates in his group, with a contagious enthusiasm which, in some of his recommendations of students, may perhaps tend to be somewhat unduly weighted with kindness. In research he has shown judgment as to what problems are important, and skill in organizing methods for their attack. He has shown rare ability in cooperating with others in research. His friends know him for his loyalty and for his sparkling personality, bubbling over with kindly humor.

PHOTOGRAPHY IN SCIENCE

ALEXANDER J. WEDDERBURN

Associate Curator, Division of Graphic Arts, Smithsonian Institution.

RESPONSE of scientists and scientific organizations to the First International Photography in Science competition was most gratifying to its sponsors, the Smithsonian Institution and THE SCIENTIFIC MONTHLY. A total of 361 entries were received from the United States, Canada, Mexico, and Ireland. In the black-and-white division, 243 prints were submitted. A total of 118 prints and transparencies were received for consideration in the color division. It is interesting to note that monochrome remains the more popular of the two media of expression in the field of scientific photography.

Photography is becoming more and more a tool of research. For various fields of investigation, ranging from astronomy to microscopic studies in biology and problems in physics involving almost infinitesimal time intervals, it has been necessary to develop different types of photographic apparatus and methods. In most sciences, where the validity of findings depends on extreme precision and accuracy, these methods are quite ingenious and have advanced far toward perfection.

The specific aim of the competition was to develop and extend the use of photography as a basic research tool as well as to aid in the dissemination of information about the various photo-science techniques.

The first, second, and third place winners in the color division were, respectively: Dr. Thomas C. Poulter, associate director, Armour Research Foundation, Chicago, Illinois; Mr. Albert M. Stover, head, Development Section, Plastics and Chemicals Division, The Glenn L. Martin Company, Baltimore, Maryland; and Mr. Ralph O. Marts, Forest Products Laboratory, Forest Service, U.S.D.A., Madison, Wisconsin.

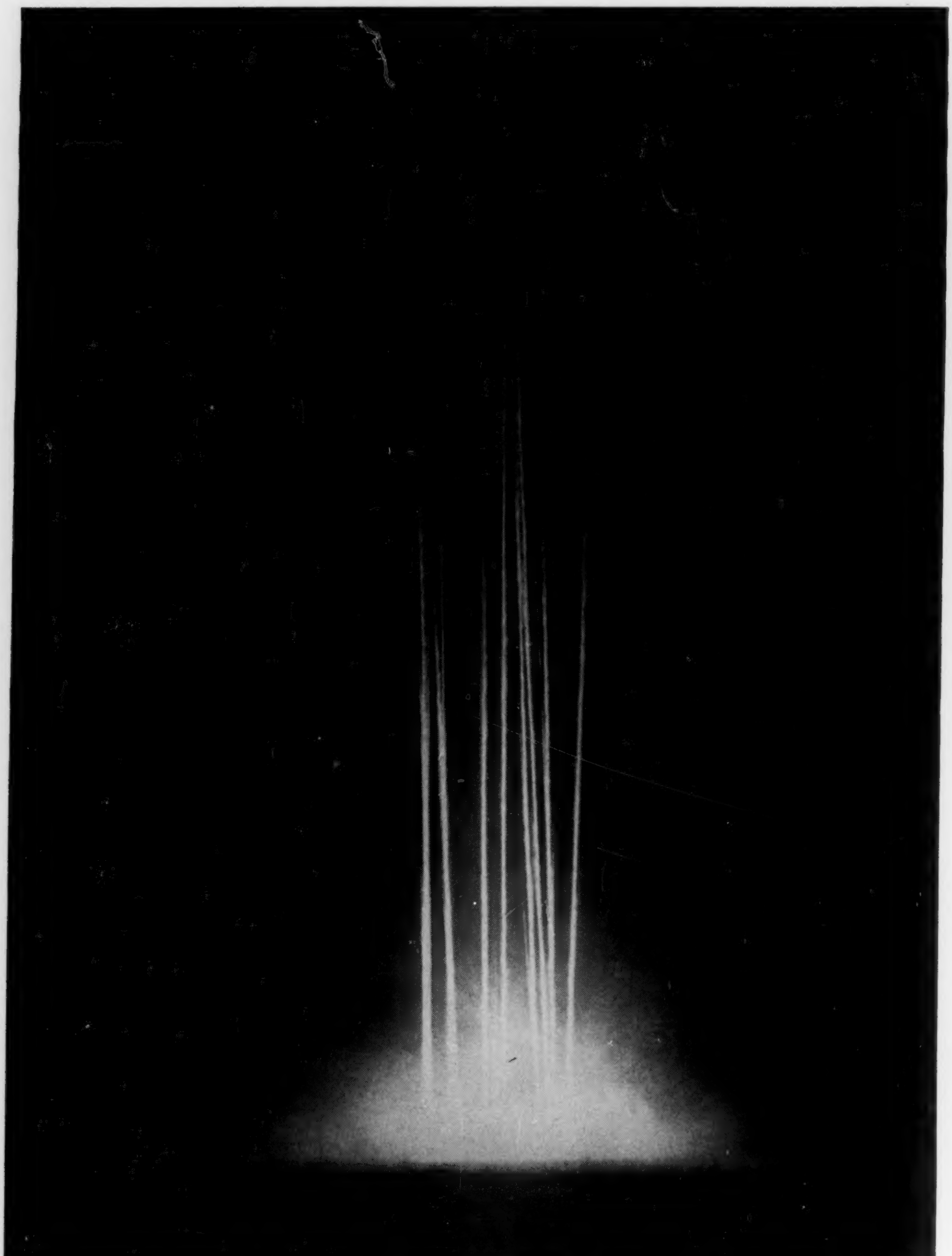
Winners in the black-and-white division were: Dr. Edwin R. Willis, Biological Laboratories, Philadelphia Quartermaster Depot, first place; Mr. V. P. Hollis, University of Minnesota, St. Paul, second place; and Mr. Harry U. Rhoads, Lambert Pharmacal Company, St. Louis, Missouri.

Members of the Judging Committee, who met October 27, 1947, were Dr. Ralph D. Bennett, technical director, Naval Ordinance Laboratory, White Oak, Maryland; Dr. K. M. Endicott, U.S. Public Health Service, Bethesda, Maryland; Mr. A. Aubrey Bodine, associate editor, *Camera Magazine;* and A. J. Wedderburn.

Entries were judged on the basis of initiative, originality, and results obtained through the use of photography. A further consideration was proficiency in the use of various photographic techniques. In view of the great number of entries that could not be removed from consideration on other grounds, final elimination was based on a critical analysis of technical excellence of presentation from the photographic standpoint. Conversely, certain of the best pictorial representations were rejected for failure to meet the requirement of originality of technique or that of unique or unusual results obtained in the particular field of science represented.

Among the techniques exemplified, many photomicrographs, electron micrographs, macrophotographs, and astrophotographs were outstanding for their technical excellence and their scientific, industrial, and military value as research tools. Sound photographic methods employed were evidenced by the excellent rendering of the correct tonal qualities in the resultant prints and color transparencies.

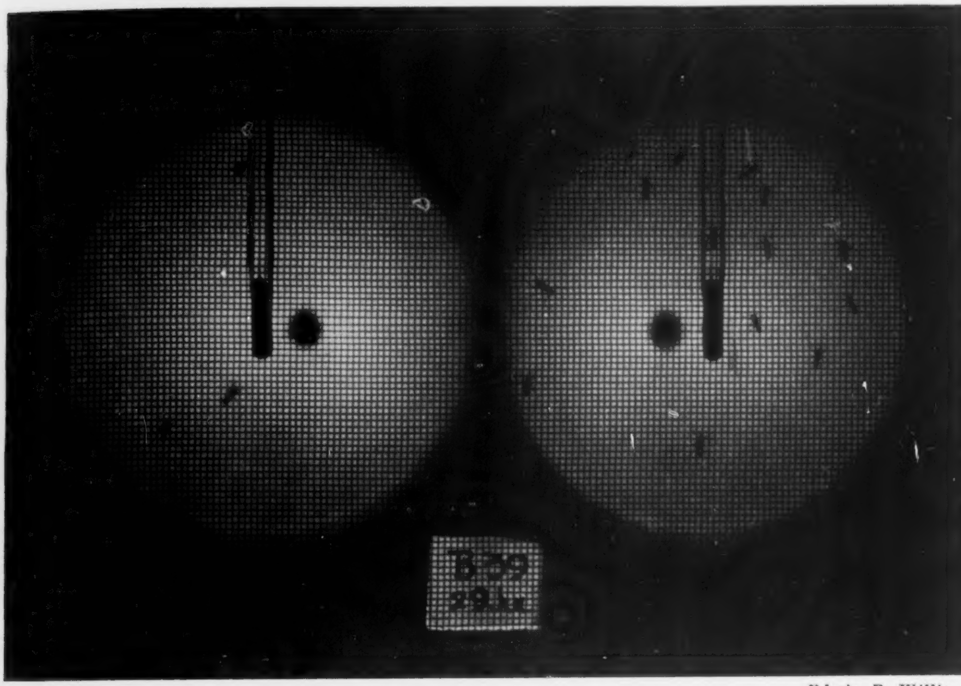
Perhaps the most significant aspect of the



Thomas C. Poulter

FIRST PLACE, COLOR DIVISION

REPRODUCED HERE IN BLACK AND WHITE, THIS PHOTOGRAPH BY DR. POULTER, ASSOCIATE DIRECTOR, ARMOUR RESEARCH FOUNDATION, SHOWS THE RESULTS OF A FIELD TEST TO DETERMINE THE SPACING NECESSARY TO PREVENT COUNTERMINING IN MONROE-EFFECT, ANTI-SUBMARINE BOMBS. THE JETS AVERAGE 250 FEET.



Edwin R. Willis

FIRST PLACE, BLACK-AND-WHITE DIVISION

DR. WILLIS, FORMERLY RESEARCH ASSOCIATE OF THE OHIO STATE UNIVERSITY RESEARCH FOUNDATION, UTILIZED PHOTOGRAPHY TO MAKE ACCURATE COUNTS OF MOSQUITOES RESPONDING TO ODORS IN HIS INSECT OLFACTOMETER. THIS PICTURE, ONE OF MANY TAKEN DURING THE COURSE OF HIS WORK IN THE DEPARTMENT OF ZOOLOGY AND ENTOMOLOGY, SHOWS THE MAJORITY OF THE INSECTS RESTING ON THE RIGHTHAND PORT OF THE OLFACTOMETER TO WHICH THEY WERE ATTRACTED BY ODOR FROM A MAN'S ARM. HALF SIZE.

competition was the almost universal use of conventional means of photographic recording. With few exceptions, which will be mentioned later, the great bulk of the photographs were produced by means that have become recognized and commonly accepted as standard photographic procedure. The electron micrograph, although comparatively new, must be classed with the above group.

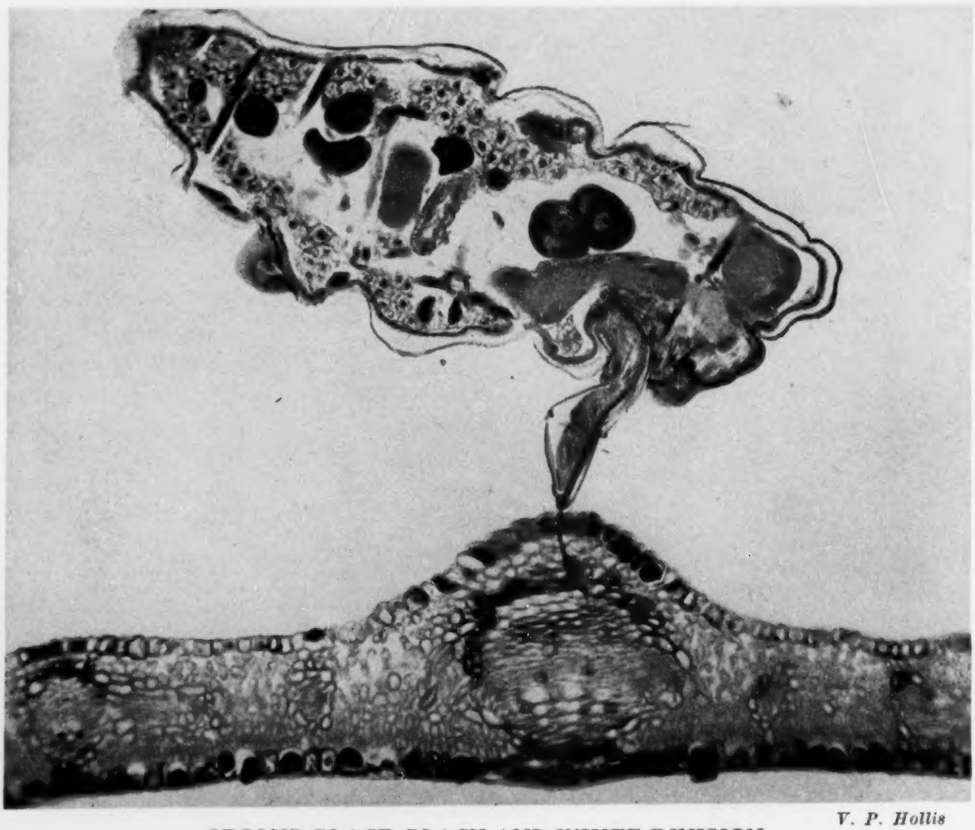
Dr. Poulter's winning print in the color division is an appropriate example of the application of photography to military usage. The picture graphically illustrates the results of a test to determine the spacing necessary to prevent countermining in Monroe-effect, antisubmarine bombs. Placed too closely together, these bombs will not produce jets. Tests were conducted to determine how closely bombs could be spaced and still produce jets (visible in picture). The jets illustrated are approximately 250 feet in height and capable of penetrating 23 inches of solid armor plate. The color photograph was made at a distance of about a quarter of a mile from the detonation point.

Mr. Stover's second-place entry in the color division, which unfortunately was not suitable for reproduction in these pages, is a series of photographs taken by means of polarized light to show the stresses developed in a plastic film being subjected to a "trapezoid tear test." The increasing area under stress clearly shows in the pictures that tensile strength rather than tear resistance is being measured, thus ruling the test out for extensible plastic film. According to Mr. Stover, the test, which is standard for meas-

uring the strength of fabric, had been proposed as a measure of the tear strength of unsupported plastic film.

The third-place-winning 35-mm. color transparency of Mr. Marts, which is also unsuitable for monochrome reproduction, is a fluorescence photomicrograph entitled "Vascular Bundle in Fern."

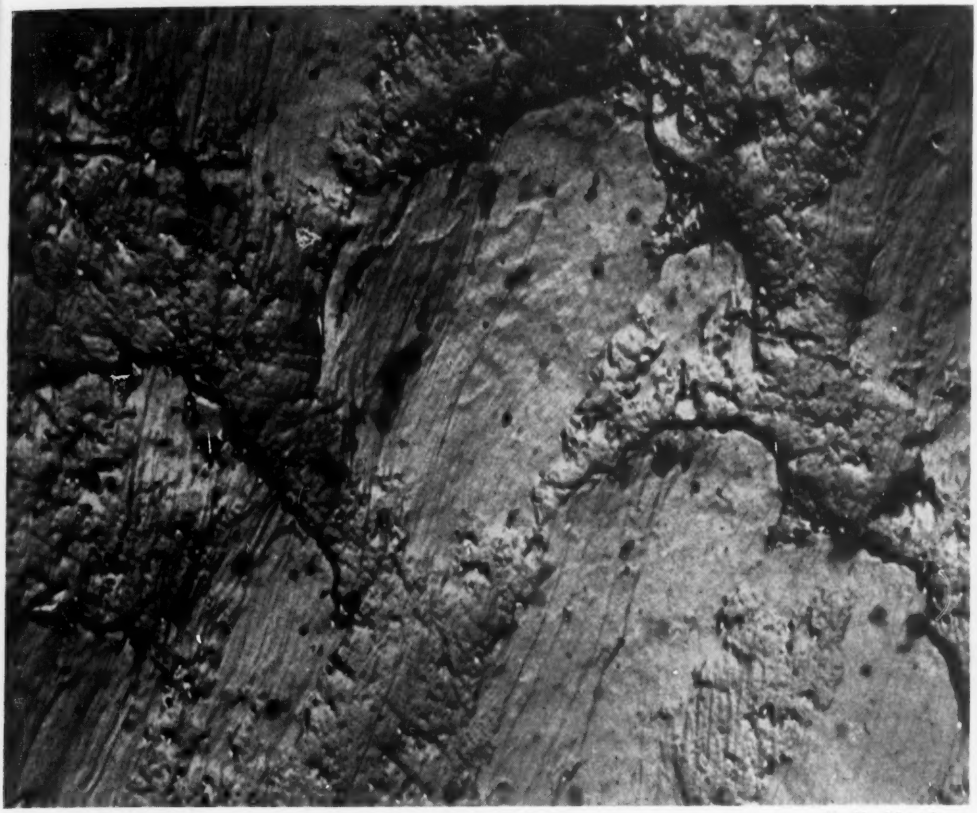
Dr. Willis' application of photography to medical research is an interesting example of how accurate data were obtained that were not otherwise available by direct visual observation because of the rapid movement of the specimens under observation. Females of the yellow-fever mosquito (*Aëdes aegypti*) are shown responding to the odor of a man's arm at the righthand port of an insect olfactometer. With the exception of the test odor in the righthand air stream, there was a close correspondence of other stimuli (temperature, relative humidity, rate of air flow, and extraneous odors) in both air streams. This photograph was the first of ten, exposed at one-minute intervals, that comprise one test in an investigation of the olfactory responses of female mosquitoes. It is of interest to note that Dr. Willis used three 25-watt Mazda lamps that, symmetrically arranged behind the two translucent funnels that formed the olfactometer ports, silhouetted the mosquitoes on the side of the wire-screen cage at the instant the exposure



V. P. Hollis

SECOND PLACE, BLACK-AND-WHITE DIVISION

THIS PHOTOMICROGRAPH IS REMARKABLE BECAUSE IT SHOWS A COMPLETE LONGITUDINAL SECTION OF A PLANT LOUSE FEEDING UPON A LEAF. MR. HOLLIS, UNIVERSITY OF MINNESOTA, NOT ONLY PICTURES THE APHID ABOVE AND THE LEAF BELOW, BUT SHOWS THE APHID'S BEAK INSERTED INTO LEAF TISSUE.



H. U. Rhoads

THIRD PLACE, BLACK-AND-WHITE DIVISION

MR. RHOADS, LAMBERT PHARMACAL CO., ST. LOUIS, PRESENTS AN ELECTRON MICROGRAPH OF HUMAN TOOTH ENAMEL SHOWING THE STRUCTURAL CONFIGURATION OF THE ENAMEL RODS AND INTERROD SUBSTANCE.

was made. In the intervals between the successive exposures, the mosquitoes were in complete darkness. This method of lighting was chosen to avoid establishing a counterattractant of heat in any part of the olfactometer other than at the ports.

The second-place photomicrograph of Mr. Hollis in the black-and-white division is illustrative of the combination of fine techniques. The photographic excellence of the print is apparent. The preparation of the longitudinal section of the plant louse is skillfully done and is unusual in that it portrays the whole aphid with its beak penetrating the leaf structure.

The electron micrograph (magnification, 23,400 ×) by Mr. Rhoads of human tooth enamel shows the structural configuration of the enamel rods and interrod substance. The view illustrated was made at an angle to the axis of the enamel rod, the tooth being dulled with Sturge chalk. Working from the print, the replica is prepared by the polystyrene silica technique of Heidenreich.

Aside from the conventional photographic processes used by contributors, there were several interesting adaptations of known techniques to specific needs in given situations, calling for originality of method to obtain hitherto unobtainable results. Additionally, processes were advanced by which the considerable expense of negative use and preparation was eliminated. In the field of

photographic research, an entry was submitted in the color division as possessing basic potentialities of a third dimensional effect through a subtractive method and means of optical image modulation.

In the category of adapted photographic techniques was a print from Dr. Harold W. Manter, Zoology Department, University of Nebraska. This photograph of the scolex and anterior segments of a tapeworm, *Ptychobothrium* species, from the intestine of a flying fish, *Cypselurus callopterus,* was made without the aid of a camera. The stained and mounted specimen on a microscope slide was projected through a Spencer microfilm reader directly on the printing paper. Although the resultant image was actually a negative one, the proper reversal was obtained in the final print by means of staining the specimen carmine red, resulting in a white subject on a black background. In this instance, white being the color of the living animal, the negative print was more natural than the specimen on the slide.

A method for the rapid recording of plating results was illustrated by Mr. Kenneth A. Wagner, instructor in botany, the University of Tennessee. The permanent records of Petri plates may be made cheaply and quickly by direct contact printing. The photogram is accomplished by placing the plates on a sheet of photographic printing paper, the exposure being made by an overhead light. The expense and time of making the usual film are thus eliminated. It should be noted that although the resulting print is actually a paper negative, positive results are obtained, the clear areas of the plate allowing passage of a greater percentage of illumination than those areas covered by the specimens. Thus the colonies that appear white on the Petri plate reproduce white on the less-exposed portions of the paper.

Of interest to entomologists, Mr. H. Lou Gibson (Honorable Mention) provided a four-picture illustration of his method of photographing insects having a true metallic silver coloration. The specimen used was *Plusiotis gloriosa,* a rare scarab native to Arizona. The bands on the elytra are rough-surfaced "mirrors," and the elytra are pale-green, enamel-like surfaces. Methods satisfactory for photographing other insects are unsuccessful with this beetle. The difficulty experienced in photographing this insect results from the recording of specular high lights and reflections of surrounding objects in the elytra. The reflection characteristics are shown in one picture where a dot-dot-dash symbol on a small card was picked up strongly in one of the silver bands and noticeably in the elytrum. The problem was solved by making a cage of lead foil around the insect, throwing light into the case, and thereby photographing the foil reflections in the bands. A "set-up" of pencils and tape were used for casting shadows on the far legs and on the near side of a supporting piece of cactus to minimize the more intense high lights. Small mirrors were used for fill-in light, and back lighting was accomplished by means of a photoflood lamp.

A measure of progress in the pursuit of three-dimensional photography without the use of viewing devices, lenticulated film, laminated screens, or other impedimenta was claimed by Mr. Ivan M. Terwilliger, of Pictures Inspacian, Santa Monica, California. His color-slide entries show a photographic sequence of four pictures (frames) exposed consecutively at standard sound motion-picture speed of 24 frames per second. The sequential frames demonstrate a modulation cycle within the picture, by which each frame is individually different, photographically and pictorially, from any other of the adjacent three. Each frame of the sequential cycle has its own individual depth of focus that is slightly different from that of any one of the other three of the same sequence. Each picture of the sequential cycle has a double focus, in which a major portion of the picture's density is sharp on the foreground images, and a minor portion of the picture's density is sharp on the background images. The ratio between the foreground-sharp-density and the background-sharp-density is different for each picture of the sequence, the

total density of each picture being the same as that of the others. Observation of the slides reveals that in each frame of the sequence the pictured background has a degree of sharpness dissimilar to the sharpness of the corresponding backgrounds in the other frames.

In connection with three-dimensional photography, it is of interest to note that another system, designated as the "Trivision" process, will be demonstrated soon at the Naval Photographic Center, Anacostia, Washington, D. C., by its inventor, Mr. Douglas F. Winnek, of Mount Vernon, New York. Mr. Winnek is now putting the finishing touches on his equipment at the Navy's Aeronautical Photographic Experimental Laboratory at the Philadelphia Navy Yard. No special seeing devices are used to obtain the illusion of depth in the Trivision process. Tiny, almost invisible ridges on the film are the basis of the invention. In the new system, ridges, or lenticulations on a transparent picture surface are applied to the film. These ridges serve as thousands of tiny recording surfaces accepting many pictures when the film is exposed in the camera. The final print is a composite of many pictures.

Navy Trivision equipment developed by Mr. Winnek includes a press for putting the ridges on film surfaces, single-lens camera, and an enlarger printer for performing a critical movement of the film during enlargement or reduction of a picture.

By use of the Winnek process, it has been predicted that three-dimension X-rays will be used to enable a surgeon to predetermine the necessary depth of incision before an operation.

The study of radioactive substances in relation to their reaction on forms of animal life was represented by a series of color prints by Miss Jane K. Glaser (Honorable Mention). Reaction of a rat to plutonium was indicated in the picture by a typical localized graying of the hair at the site of the deposition. In another print tumors of the intestine were visible following ingestion of radioactive yttrium. A third in the color series represented rat lung tumors following the inhalation of radioactive cerium.

An informative photomicrograph by Mr. Albert J. Oliver, of the Radiation Laboratory, University of California, Berkeley, depicted a nuclear event recorded directly in a photographic emulsion. The emulsion had been bombarded in the 184-inch Berkeley cyclotron with a beam of alpha particles having an energy of 80 MeV. The plane of the emulsion was tangent to the beam. The photographic plate, a portion of which was the subject of the entry, was not exposed to light. The silver grains were deposited by ionization caused by high-energy charged particles. The tracks of the charged particles were formed directly in the emulsion. In the photographed area, one of the alpha particles passed into the edge of the emulsion, and its path is easily traceable by the track of developed grains.

In the field of medical research, Mrs. Muriel C. MacDowell, of the Department of Pathology, Long Island College of Medicine, Brooklyn, entered four composite photomicrographs from which detailed study of entire kidney units is possible. The MacDowell entry is one of the best examples of the close relationship of science and photography to the rapidly expanding visual-education field.

One of the fine composites was a spodogram showing distribution of mineral ash in the rat nephron following the administration of calciferol. A nephron (the structural and functional unit of the kidney) from a rat that was excreting large amounts of Ca in the urine following administration of the calciferol (Vitamin D) was isolated by microdissection under water from kidney tissue macerated in acid. After mounting on a slide, the specimen was incinerated at a temperature of 500° C. for one half hour, thus destroying all organic substances and leaving in its original structural pattern the nonvolatile oxides of the protoplasmic minerals Mg, Na, K, Li, and, in great excess as a result of the experimental procedure, Ca. Successive overlapping photomicrographs of 100× magnification were taken

under constant conditions of illumination (dark field by means of cardioid condensor), exposure, and development. From the overlapping prints, the proper contiguous portions were cut and matched to form, when mounted, an exact composite photomicrograph of the entire kidney unit. In the final print, the varying content of mineral, mostly Ca, is clearly visible in the various portions of the nephron. Heavy deposits are seen in the proximal convolution (recorded by 15 exposures); in the ascending limb, which required 7 exposures, only traces are seen, which increase in the distal convolution to disappear in the branching collecting tubule.

Of the many entries submitted, a very small number were rejected because of inappropriate subject matter. The accepted photographs, which were shown at the Smithsonian Institution during November, will be part of the International Science Exhibition of the A.A.A.S. at Chicago, December 26–31.

From there the exhibit will move to the Buhl Planetarium in Pittsburgh; the next stop will be at the Cranbrook Institute of Science, Bloomfield Hills, Michigan. Requests for displaying these pictures at other institutions should be addressed to the editor of THE SCIENTIFIC MONTHLY.

The Second International Photography in Science competition is scheduled for exhibition in September 1948 in the U. S. National Museum of the Smithsonian Institution. Further details will be announced later.

HONORABLE MENTION WINNERS

Black-and-White Division:

DR. F. A. HAMM, Central Research Laboratory, General Aniline & Film Corporation, Easton, Pa.: electron micrograph of crystals of heliogen blue pigment.

DR. CORNELIUS B. PHILIP AND N. J. KRAMIS, Rocky Mountain Laboratory, U.S.P.H.S., Hamilton, Mont.: photomicrograph of the chigger vector of tsutsugamushi, a larval mite mounted and cleared in polyvenal alcohol.

MR. THOMAS CARVER, Physics Department, Harvard University, and DR. FRANK H. J. FIGGE, Department of Anatomy, University of Maryland School of Medicine, Baltimore: a series of photographs of a hypospray jet in various stages of ejection.

MR. H. LOU GIBSON, Eastman Kodak Company, Rochester, N. Y.: series of photographs of *Plusiotis gloriosa* beetle to show technique of eliminating reflections from the elytra.

DR. ROBERT SCHREK, Veterans Administration Hospital, Hines, Ill.: photomicrograph series of cells under dark-field illumination.

Color Division:

MISS ALICE ELIZABETH SUTHERLAND, Research Division, Callaway Mills, La Grange, Ga.: photomicrograph of cross section of yarn from azlon cotton fiber.

MISS JANE K. GLASER, Argonne National Laboratory, Chicago, Ill.: tumor masses in soft tissue of rabbit from primary bone tumor.

MR. BRADFORD B. UNDERHILL, Ordnance Research Laboratory, Pennsylvania State College: multichannel oscillograph record.

MISS INES V. DEGRUY, Southern Regional Research Laboratory, U.S.D.A., New Orleans, La.: photomicrograph of cotton fiber swelled to show ballooning; photomicrograph of synthetic resin polymerized in wood.

NATURE'S GREATEST EXPLOSIONS: SUPERNOVAE*

N. U. MAYALL

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TO UNREPENTANT sinners an atomic-bomb explosion may seem like the height of the Lord's wrath, but to unhallowed astronomers the real hell-fire of the universe is a *supernova*. In this bit of astral argot the Hollywoodian prefix, "super," needs no explanation; and "nova" is a traditional astronomical term meaning a new or temporary star. It is used to describe the sudden flare-up of stars, previously unknown or obscure, which for a time blaze with thousands of times their former brilliance in the case of ordinary novae, and millions of times in the case of supernovae. For reasons not yet fully understood, in novae an explosive process gains the upper hand over the normal and stable means whereby stars maintain their energy output. In an ordinary nova, the explosive outburst seems to be a surface phenomenon that generally results in the expulsion of a star's outer, or atmospheric, layers. In a supernova, however, the explosive process probably operates throughout most of the star, and results in blowing off a large part of its mass, and perhaps in converting much mass into energy.

The most luminous of the supernovae have been observed to shine with a brightness of 600 million suns, and at maximum light they pour out energy at a rate that staggers the imagination: tens of millions of times the solar rate. Since the latter is a truly astronomical number, 5×10^{23} horsepower (10^9 = 1 billion), even the largest earthly packet of energy—the atomic bomb—pales into insignificance alongside a supernova. For arithmeticians, calculation shows that one supernova is worth about 10^{29} atomic bombs. Thus, on the cosmic scale, science's greatest achievement in releasing energy hardly amounts to so much as a gleam in a firefly's lantern.

In the giant stellar system where the sun is but an average star among many billion others, only three supernovae are known to have occurred during the past thousand years: in A.D. 1054, 1572, and 1604. The supernova phenomenon is therefore a rare one, and the odds are comfortingly close to zero that the sun may become a supernova and thereby burn to crisps its encircling planets.

The three supernovae observed in our Milky Way stellar system, the Galaxy, are of exceptional interest because, in the astronomical sense, they appeared in our own front yard, at distances of the order of only several thousand light-years (1 light-year = 6×10^{12} miles). At such distances the tremendous luminosity of a supernova makes it a really spectacular celestial object. For example, a supernova 100 million times the luminosity of the sun, if distant 5,000 light-years, would appear as bright as the planet Venus at maximum brilliance. The three known galactic supernovae therefore excited much interest in their time, and there is little doubt that, if another one flared up at the present time, it would excite world-wide wonder, make much work for astronomers, and much money for astrologers.

The first known galactic supernova—that of 1054—is perhaps most interesting, because the debris of its explosion is easily seen today, even in a small telescope, as a small oval, cloudlike patch, widely known as the Crab Nebula in Taurus (Fig. 1). The 1054 supernova, however, seems to have been observed only in the Orient, casually by the Japanese, and more extensively by the Chi-

* The author wishes to express his gratitude and appreciation to Dr. W. Baade, of the Mount Wilson Observatory, who kindly provided the illustrations for this article and who has done much to place our knowledge of supernovae on a sound observational basis.

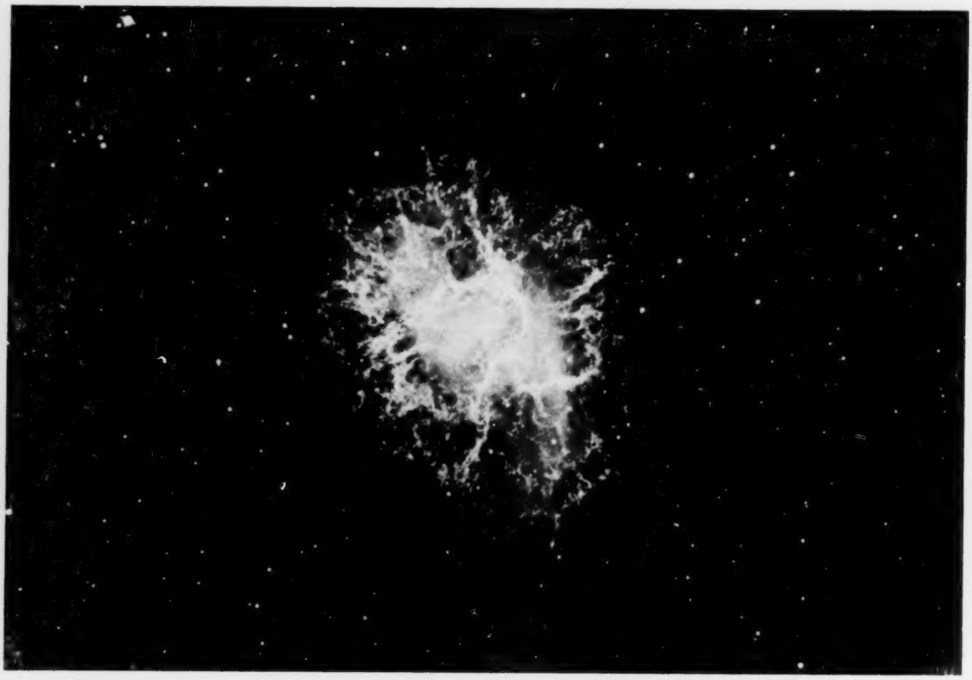

FIG. 1. THE CRAB NEBULA IN TAURUS

THIS PHOTOGRAPH, TAKEN IN RED LIGHT, SHOWS THE GASEOUS REMNANT THROWN OFF BY THE SUPERNOVA OF A.D. 1054. THE SHREDDED, FILAMENTARY STRUCTURE SUGGESTS OUTWARD MOTION, AND THE EXPANSION HAS BEEN MEASURED BY COMPARISON OF PHOTOGRAPHS TAKEN AT DIFFERENT TIMES. SUCH MEASUREMENTS, WHEN COMBINED WITH SPECTROSCOPIC OBSERVATIONS, LEAD TO A WELL-DETERMINED DISTANCE OF 5,000 LIGHT-YEARS. THE GASES COMPOSING THE NEBULA (OXYGEN, HYDROGEN, HELIUM, NEON) ARE IN A HIGHLY RAREFIED STATE, AND THEY PROBABLY ARE EXCITED TO LUMINESCENCE BY AN EXTREMELY HOT STAR NEAR THE CENTER, TOO FAINT TO SHOW IN THE REPRODUCTION. THIS EXCITING STAR, WHICH MAY HAVE A TEMPERATURE OF 500,000° C. AND A SIZE OF ONLY 2,000 MILES, IS THE ONLY KNOWN STELLAR VESTIGE OF A SUPERNOVA OUTBURST.

nese. The latter saw the supernova in daylight for several weeks, and in the night sky for nearly two years, when it faded from their unaided vision. Strange to relate, no European records of the apparition have yet turned up, possibly because popular interest in the new star was overshadowed at that time by Lady Godiva's famous horseback ride.

The other two galactic supernovae—those of 1572 and 1604—are commonly associated with two of the most famous names in astronomy, Tycho Brahe and Johannes Kepler. Tycho's nova of 1572 almost rivaled Venus at her brightest, and Kepler's of 1604 slightly exceeded Jupiter at his best. Both supernovae were observed for more than a year, and their position and brightness were systematically recorded. Although the observations were crude by modern standards—being made before the invention of the telescope in about 1609—they are, nevertheless, invaluable in the comparative study of supernovae found in other and much more distant parts of the universe.

To extend the study of supernovae from local to truly remote regions, the search for them needs to be pushed outside the Galaxy into the boundless firmament of the extragalactic nebulae. These are stellar systems, often called "Island Universes" or simply "galaxies," which are scattered more or less uniformly in space with an average separation of several million light-years. Because of these vast distances, supernovae in galaxies are seldom conspicuous objects (Fig. 2). A supernova 100 million times brighter than the sun, for example, would appear of magnitude

twelve or seventeen at distances of 10 or 100 million light-years, and its detailed study would strain the resources of the largest telescopes. The latter, however, would justify their cost if a supernova should occur in one of the dozen nearest stellar systems that group around the Galaxy at distances ranging from 90,000 light-years, for the Magellanic Clouds, to 750,000 light-years for the spiral Andromeda Nebula, Messier 31. It was therefore somewhat of a tough break for astronomers when a supernova did appear in M31 in 1885, when astronomical photography and spectroscopy were in their infancy, and before the modern powerful telescopes were in operation. This supernova, known as S Andromedae, was nevertheless an exciting object, even in telescopes of moderate size, for it flared up close to the nucleus of the spiral, and at maximum light reached the seventh magnitude.

Discoveries of supernovae in galaxies were infrequent and fortuitous until about 1929. In that year a systematic search was initiated, first, at the Mount Wilson Observatory as a patrol of the great cluster of several hundred nebulae in Virgo and, later, in 1936, on Mount Palomar (the site of the 200-inch telescope) as a well-planned and extensive campaign to photograph regularly 150 fields. These surveys more than doubled the list of supernovae in distant galaxies and by 1942 brought the total to 40, a number sufficient to indicate with some reliability that, on the average, one supernova may be expected to appear in a galaxy every 500–600 years. Supernovae in the cosmic sense are therefore rare birds.

The extensive Mount Wilson and Palomar observations of supernovae in galaxies showed that the typical behavior is a sudden rise to maximum light, a brief lingering around the maximum, and then a steady decline to disappearance. This light variation, when properly calibrated, is called the light curve, and it gives in concise form most of the basic information relating to supernovae (Fig. 3). The three natural divisions of it, rise, maximum, and decline, are remarkably similar for the brightest supernovae that have been most completely observed. The light

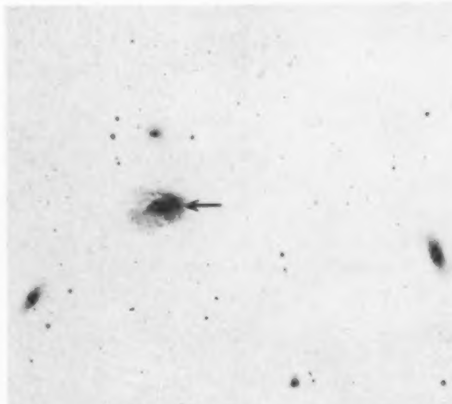

FIG. 2. A DISTANT SUPERNOVA

THE ARROW POINTS TO THE FAINT IMAGE OF A SUPERNOVA THAT OCCURRED IN 1936 IN THE SPIRAL NEBULA NGC 4273. THE SPIRAL IS DISTANT 8 MILLION LIGHT-YEARS AND IS A MEMBER OF THE GREAT CLUSTER OF HUNDREDS OF SIMILAR EXTRAGALACTIC NEBULAE IN VIRGO, SEVERAL OF WHICH APPEAR IN THE PHOTOGRAPH. THE SUPERNOVA WAS NEARLY TWO MONTHS PAST MAXIMUM WHEN THE PHOTOGRAPH WAS TAKEN, WAS OF ABOUT THE SIXTEENTH APPARENT MAGNITUDE, AND WAS SHINING WITH A LUMINOSITY EQUIVALENT TO THAT OF SEVERAL MILLION SUNS.

curves also indicate that probably there are two classes of supernova: Type I, the brightest, which average 100 million times brighter than the sun; and Type II, which average one-thirtieth as bright as Type I. Little is known about supernovae of Type II, except that they more closely resemble a normal nova than they do a supernova of Type I.

As might be expected, data regarding the rise are quite scanty, for the rate is so great, and the circumstances of observation generally so unfavorable, that the chances of recording a large part of the ascending branch are very slim indeed. Nevertheless, there is good evidence that a supernova rises from obscurity to its blazing maximum in 10 days or less. The increase in brightness is at least twenty magnitudes, or by a factor not less than 10 million times the initial luminosity. The supernova then hovers around maximum light for a week or so, when it shines with a whitish hue and with minor or no fluctuations. While at maximum, the supernova is comparable in luminosity with the stellar system in which it is located, although, because of the appreciable disper-

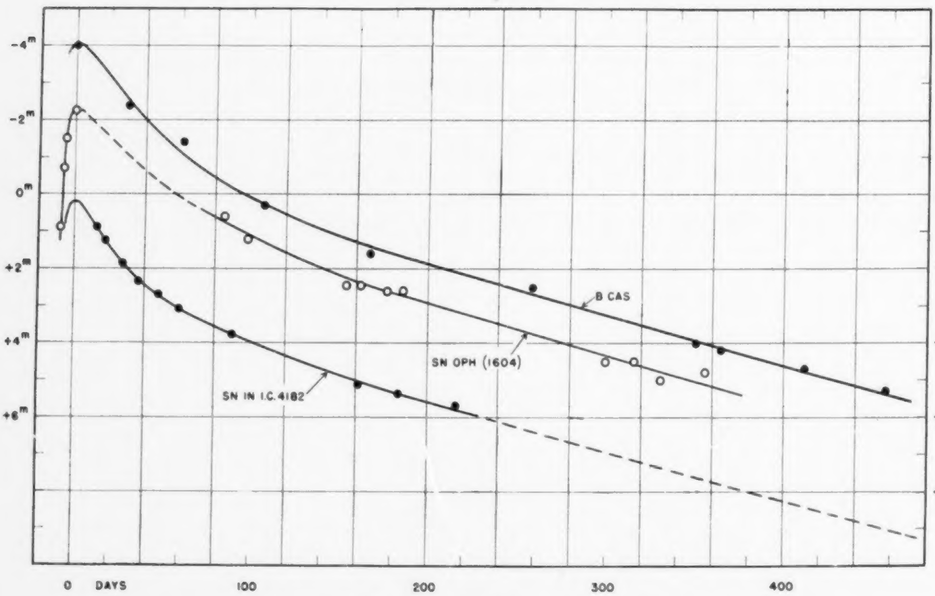

FIG. 3. LIGHT CURVES OF THREE SUPERNOVAE OF TYPE I

THE UPPER TWO CURVES ARE FOR TYCHO'S STAR OF 1572 (B CASSIOPAEIA) AND FOR KEPLER'S STAR OF 1604 (THE SUPERNOVA IN OPHIUCHUS). THE OBSERVATIONS IN BOTH CASES (*dots and circles*) WERE MADE BEFORE THE INVENTION OF THE TELESCOPE, AND THE SCALE OF APPARENT BRIGHTNESS ON THE LEFT REPRESENTS A SKILLFUL CALIBRATION OF THE OLD QUALITATIVE ESTIMATES. ON THE SAME SCALE, VENUS AT MAXIMUM IS CLOSE TO −4, JUPITER TO −2. THE LOWER CURVE IS FOR THE BRIGHTEST KNOWN MODERN SUPERNOVA THAT OCCURRED IN THE NEARBY DWARF SYSTEM IC 4182 (FIG. 5), AND ITS SCALE OF APPARENT BRIGHTNESS IS ON THE RIGHT. THE CHIEF POINT TO BE NOTED IS THE CLOSE SIMILARITY OF ALL THREE CURVES, ESPECIALLY AFTER THE HUMP.

sions in luminosity of supernovae and of galaxies, either one or the other may predominate by several magnitudes.

When the declining phase sets in, the supernova at first dims fairly rapidly, noticeably reddening at the same time. The fading, however, proceeds more slowly as time goes on, and, after approximately 3 months, the supernova loses light each day at the rate of 3.2 percent. In the best-observed case, this rate was uniformly maintained for more than 500 days, and other less completely observed supernovae substantiate the same remarkable linearity in the descending branch of the light curve (Fig. 4). The phenomenon is in marked contrast with the corresponding part of an ordinary nova's light curve, which often shows large and irregular variations.

In all cases of supernovae discovered in stellar systems beyond the Milky Way, the stars apparently vanish completely, without leaving any traces of the explosions (Fig. 5). But this fact does not necessarily mean that the star entirely transforms itself into radiation; it is more probable that, because of the great distances and our limited telescopic power, we are unable to photograph the products of celestial spontaneous combustion. In order to find out something about the remains of nature's greatest explosions, it is necessary to consider the cases of the three known galactic supernovae that have already been mentioned.

ALTHOUGH the Crab Nebula has been known for more than 200 years, it is only within the past 25 years that it has been recognized as the gaseous remnant of the supernova of 1054. The story of its identification is too long to tell here; suffice it to say that the ancient oriental chronicles and modern observations leave no reasonable doubt that the nebula originated in the outburst. Both direct photographs and spectro-

grams show that the nebula is expanding, as would be expected from an exploding star, and as has been observed in several ordinary novae. But of paramount importance is the presence, near the center of the nebula, of a certain sixteenth-magnitude star that *may* be the old supernova, 22 magnitudes fainter than at maximum. It is necessary to say "may" because the crucial observations are difficult to make, even with the largest telescopes. By the few tests that it is possible to make, however, there is good reason to regard this faint star as the only known end product of a supernova outburst. The properties of this faint star then become of surpassing importance, but unfortunately we know little of them beyond what can be theoretically inferred from the characteristics of the expanding gases that the star excites to luminescence. If the theoretical interpretation is correct, this faint star is an extraordinary object, having a temperature of the order of 500,000° C., a total luminosity 30,000 times, a density 180,000 times, and a radius only one-fiftieth that of the sun.

Since the positions of Tycho's star of 1572 and Kepler's of 1604 were determined as accurately as possible while the stars were visible, it is natural to wonder whether any traces of these stellar explosions can be located at the present time. The search has proved successful only in the case of the supernova of 1604. Photographs taken in red light with the 100-inch Mount Wilson reflector disclosed the presence, very close to the location of Kepler's star, of a number of faint, broken knots and filaments of a fan-shaped nebulosity (Fig. 6). Its spectrum, which is very difficult to record, closely resembles that of the Crab Nebula. All the indications are that the fan-shaped, brightest area is only one part of a larger mass, which future observations are expected to show is expanding. No stellar vestige has yet been identified, although there is a suspicious eighteenth-magnitude star at the point of the fan. The supernova appeared in the Milky Way in a region rich in faint stars highly reddened by interstellar dust, and the identification of a very faint hot star, normally blue, is a formidable job.

Although the search for the remains of the supernova of 1572 has been pushed with equal vigor and skill, nothing has been found in the location of Tycho's star. Unfortunately, because of its type of mounting, the 100-inch reflector cannot be pointed to the far-northern position of the old supernova. But the 200-inch telescope will not be thus restricted, and the odds are about even that

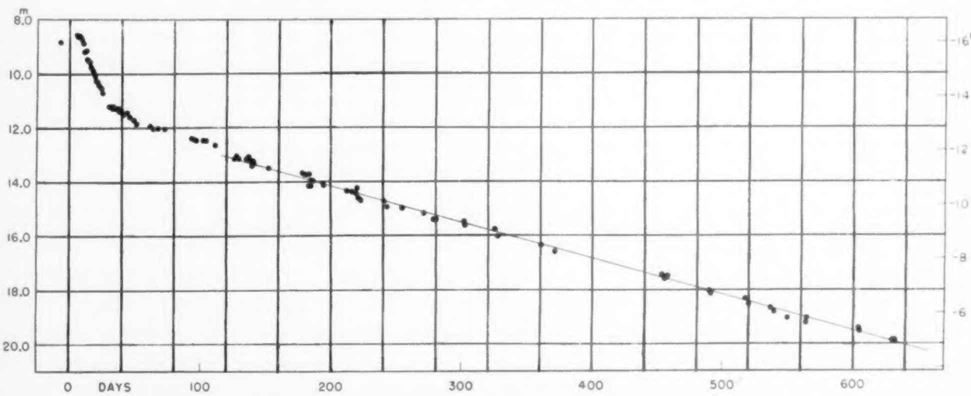

FIG. 4. LIGHT CURVE OF THE SUPERNOVA IN IC 4182

DETERMINED FROM MODERN PHOTOGRAPHIC OBSERVATIONS, THE SCALE OF APPARENT BRIGHTNESS (m) ON THE LEFT SHOWS THAT THIS SUPERNOVA WAS FOLLOWED FROM ITS MAXIMUM TO NEARLY $m = 20$, WHICH IS NEAR THE LIMIT OF MEASUREMENT WITH THE 100-INCH TELESCOPE. THE SCALE OF LUMINOSITY (M) ON THE RIGHT SHOWS THAT, AT MAXIMUM, THE SUPERNOVA REACHED AN ABSOLUTE MAGNITUDE OF $M = -16$, OR SOME 600 MILLION TIMES AS BRIGHT AS THE SUN, WHICH, ON THE SAME SCALE, HAS $M = +5$. A NOTEWORTHY FEATURE OF THE LIGHT CURVE IS THE UNIFORM RATE OF DECLINE, MAINTAINED FOR MORE THAN 500 DAYS.

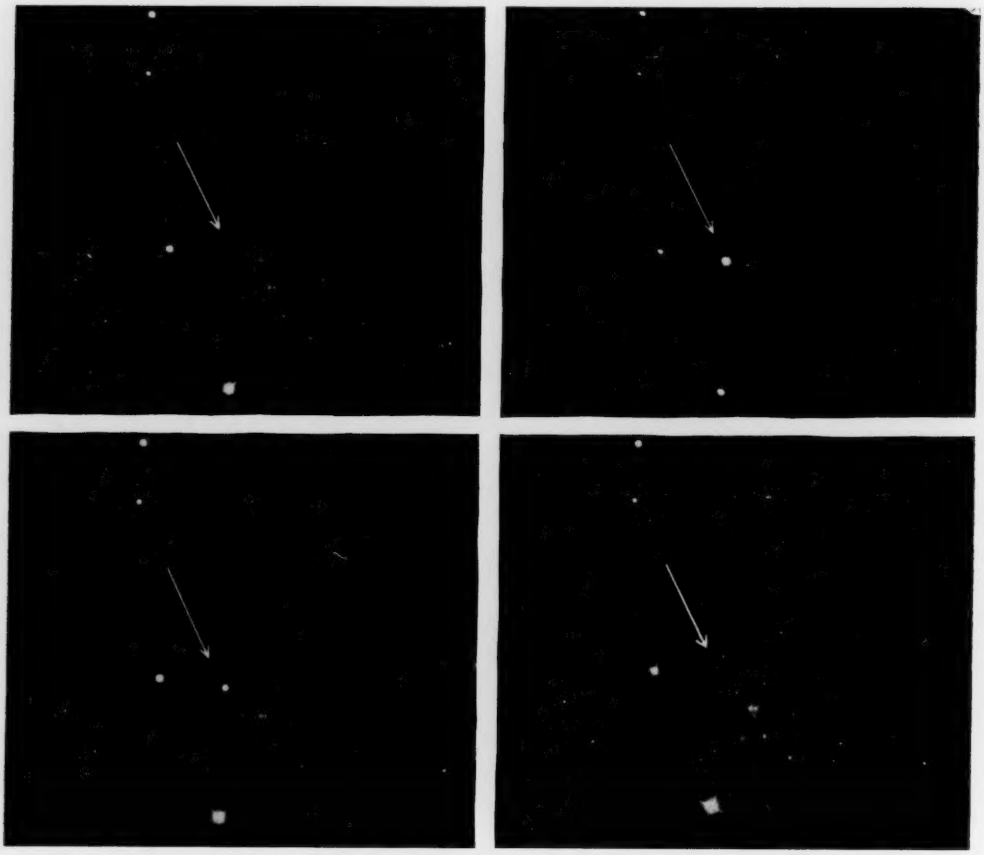

FIG. 5. THE PICTURE HISTORY OF A SUPERNOVA

(1) *Upper left*, OBSCURITY, MARCH 8, 1937; (2) *upper right*, MAXIMUM, SEPTEMBER 10, 1937; (3) *lower left*, DECLINE, APRIL 1, 1938; (4) *lower right*, OBLIVION, JANUARY 19, 1940. THESE PHOTOGRAPHS RECORD THE OCCURRENCE OF THE BRIGHTEST KNOWN SUPERNOVA, MAXIMUM LUMINOSITY EQUAL TO 600 MILLION SUNS, THAT OCCURRED IN 1937 IN THE DWARF EXTRAGALACTIC STELLAR SYSTEM IC 4182, DISTANCE 3 MILLION LIGHT-YEARS. IN (2) THE PARENT STELLAR SYSTEM DOES NOT SHOW BECAUSE THE EXPOSURE TIME WAS PURPOSELY SHORTENED TO AVOID OVEREXPOSURE OF THE SUPERNOVA. IN (1) AND (4), ALTHOUGH MANY STELLAR CONDENSATIONS APPEAR, THERE IS NO TRACE OF THE SUPERNOVA BEFORE AND AFTER ITS OUTBURST.

a search to fainter limits will uncover the shredded skeleton, or shade, of the strange star of 1572.

Of comparable importance with the light curve is the spectrum of a supernova (Fig. 7). But here, too, the difficulties of observation are considerable, and up to the present only one good series of spectrographic observations has been obtained of a supernova. This was one that had the highest recorded luminosity of 600 million suns and attained nearly the eighth apparent magnitude, although at a distance of 3 million light-years. Less complete and fragmentary spectroscopic records of other and fainter supernovae have been obtained, and all the material indicates that spectra of supernovae are astonishingly similar at corresponding parts of their light curves. Beyond that fact, however, a typical spectrum is almost an unsolved puzzle. It is not even established, for example, whether the spectrum consists wholly of very wide emission bands, of a continuous spectrum, or of a combination of both. Only two radiations have definitely been identified: two emissions in the red, due to neutral oxygen atoms, which are characteristic of a gas of very low density. These emissions are much

narrower than other broad-banded features in the supernova spectrum, and they make their appearance some 180 days after maximum. These facts—smaller width and time of appearance—suggest comparison of supernovae with ordinary novae, which show similar radiations at a time when the ejected gaseous envelope has expanded into a shell of great tenuity, much larger than the initial stellar atmosphere.

The most promising direction to take for the interpretation of the mysterious supernova spectrum may be to proceed from the fairly well-understood spectrum of a normal nova, through that of supernovae of Type II to that of Type I. The reason for such a procedure is that fairly recent spectroscopic observations of a supernova of Type II, followed from maximum to 76 days afterwards, show characteristics that may be intermediate between normal novae and supernovae of Type I. The evidence suggests that a normal nova is but a vest-pocket edition of a supernova of Type II; but whether a supernova of Type I is a full-folio edition of a supernova of Type II remains to be seen. At the present stage of our knowledge of supernovae, it is difficult to know whether to stress the differences or the similarities between supernovae and ordinary novae.

Theories of the supernova phenomenon plunge into a realm as bewildering and interesting as Alice's Wonderland. Such cataclysmic events as stellar collisions, annihilation, and collapse have been postulated in order to account for the fantastically large amounts of energy liberated. The general conclusion of much intellectual abstraction is that the causes for supernova outbursts probably are to be sought within the stars themselves, and not in some external happening such as a collision or close approach of two stars. Moreover, since probably there is one stellar remnant known, and more may be found, complete conversion of a star into energy (annihilation) seems to be ruled out. The elimination leaves the field to the collapse process, which has some highly diverting aspects.

At some point in its life history, a star approaches the condition of exhausting its fuel

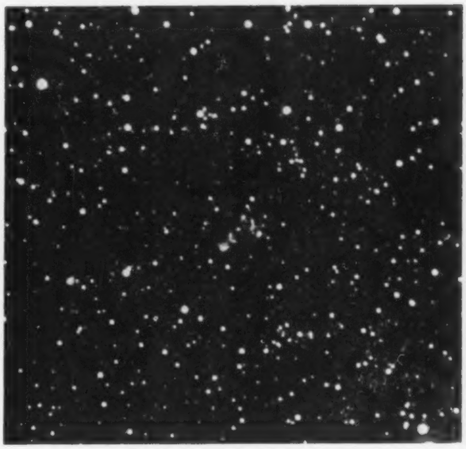

FIG. 6. A SUPERNOVA'S REMAINS

THIS PHOTOGRAPH, TAKEN IN DEEP RED LIGHT IN ORDER TO PENETRATE THE INTERSTELLAR DUST HAZE IN THE MILKY WAY, SHOWS THE SPARSE GASEOUS REMAINS OF KEPLER'S SUPERNOVA OF 1604, THE LAST KNOWN SUPERNOVA IN THE GALAXY. FUTURE OBSERVATIONS ARE EXPECTED TO SHOW THAT THE GASEOUS FILAMENTS ARE MOVING OUTWARD FROM THE CENTER OF THE EXPLOSION, AS IN THE CRAB NEBULA, BUT UNTIL, OR UNLESS, SUCH MEASUREMENTS ARE MADE, THE DISTANCE TO THIS OLD SUPERNOVA PROBABLY WILL BE INDETERMINATE, BECAUSE OF UNCERTAINTY REGARDING THE AMOUNT OF DIMMING PRODUCED BY THE HAZE OF THE INTERSTELLAR DUST.

supply, which in most cases probably is the predominately abundant hydrogen. With depletion of its source of energy, the star contracts and grows smaller, dimmer, and denser. But the process may or may not be uniform, or lead to a stable configuration, depending upon the amount and composition of matter in the star. Matter in an exceedingly dense state, wherein the atoms are entirely stripped of their electrons and are thereby packed very close together, is called degenerate. According to one theory of degenerate matter, there may be a critical range of stellar masses such that, during the contraction phase, degenerate cores of exceedingly high density may develop. For sufficiently high densities, the stripped nuclei of hydrogen atoms (protons) may combine with electrons to form neutrons. The combination would result in a sudden decrease in the central pressure, and the star would collapse with the release of a prodigious amount of energy. In another formulation of the problem, the

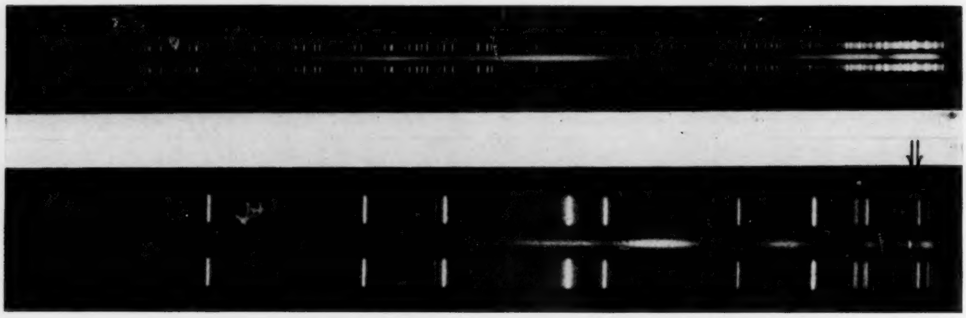

FIG. 7. THE SPECTRUM OF A SUPERNOVA

Above, 9 DAYS; *below,* 225 DAYS PAST MAXIMUM. IN THESE REPRODUCTIONS OF SPECTROGRAMS, RED IS TO THE RIGHT, AND BLUE-VIOLET TO THE LEFT; COMPARISON SPECTRA (THE TWO SETS OF SHORT VERTICAL LINES ON EITHER SIDE OF THE SUPERNOVA SPECTRA) ARE OF IRON AND NEON (*above*), AND OF MERCURY AND ARGON (*below*). ALTHOUGH THE BROAD BANDS IN THE SUPERNOVA SPECTRA HAVE NOT BEEN CERTAINLY IDENTIFIED, THEIR GREAT WIDTHS SUGGEST A STELLAR ATMOSPHERE EXPANDING WITH HIGH VELOCITY, UP TO 4,000 MILES PER SECOND. THE ONLY DEFINITELY IDENTIFIED RADIATIONS ARE TWO NARROW ONES IN THE RED (*indicated by arrows on right*) THAT ARE CHARACTERISTIC OF NEUTRAL OXYGEN SHINING AS A HIGHLY RAREFIED GAS.

decisive factors are rotational instability and central density. If the star reaches a certain critical central density before it begins to throw off matter by rotation, the central temperature can become high enough (several billions of degrees Centigrade) to produce nuclear reactions which absorb, rather than emit, energy at an enormous rate. The result is that the internal pressure due to radiation is no longer maintained, and thus the balance between the opposing forces of radiation and gravitation is destroyed. The change from emission to absorption takes place so fast—the computed time is only 100 seconds!—that the star collapses and releases its gravitational energy on a stupendous scale.

Just how far the collapse of a star can proceed is a question of much interest. If the stellar remnant of the A.D. 1054 supernova is an example, it has a computed radius of about 2,000 miles. Although this value may seem small for a star, it is by no means so small as some of the limiting sizes computed in theories of degenerate neutron cores. One calculation, for example, led to a minimum size of only a few miles, which if significant would have some curious consequences. For a stellar mass to occupy so small a space would imply an enormous Einstein red shift of the star's radiation—right out of this world. The properties of space around such an object would be so drastically altered by the strong gravitational field that light could neither enter nor leave the vicinity. If such is the fate of a supernova, we have the singular phenomenon of the greatest outburst in the universe ending as practically nothing. It is as if a star, after a cosmic binge, suffers remorse, and then achieves the ultimate in oblivion by crawling into a hole and pulling the hole after it.

INTRAVENOUS INJECTIONS

CARL A. DRAGSTEDT

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THE administration of drugs, or of fluids such as saline, plasma, and blood, by direct injection into the veins is now such an everyday occurrence that little thought is given either to the vicissitudes involved in adding this technique to the physician's repertoire or to the numerous by-products of information that pursuit of this objective produced. There are a number of very excellent reviews of the development of this procedure presented in more or less orthodox historical fashion, to which the interested reader is referred for detailed accounts, the present review being an attempt to survey the story from the following points of view: the motives that inspired this procedure; the methods employed; the difficulties, problems, and reactions encountered; and, finally, the lessons learned incidental to the prosecution of this procedure.

According to Brown, the earliest purpose for which intravenous injections were made was the post-mortem injection of vessels made to facilitate anatomical dissection and study. This was done by Mondinus as early as 1316 at Bologna and developed to a greater extent by Silvius, Eustachius, and Vesalius. Although the scientific employment of this procedure would seem likely to presage the general practice of embalming the dead by vascular injection, this procedure does not appear to have been employed to any extent until after intravenous injections in the living had been studied rather extensively.

It seems very probable that the first purpose conceived as requiring intravenous injection in living animals or man was the transfusion of blood, but as to whether blood was actually transferred from one animal to another by this method until after the feasibility of this route was demonstrated by using other agents is uncertain. Omitting the ancient references by Ovid and others to the possibility of blood transfusion, as being so vague, on the one hand, and so barren of results in the way of recorded efforts to put the hints into practice, on the other, as to leave grave doubts that the references are any more than literary extravagances, it seems possible that the Italian philosopher Ficinus was the first (in 1489) to suggest that the blood of a healthy young man might be given to the aged for its rejuvenating effect. Villari, in his *Life and Times of Girolamo Savonarola,* recounts the purported attempt of a Jewish physician to save the life of Pope Innocent VIII in 1492 by transfusing the blood of three young boys into the aged and sick pontiff. Various historians, who take the position that the idea of blood transfusion could not have arisen prior to the knowledge of the circulation of the blood (Harvey, 1628), discount this report by asserting that it is most probable that the blood drawn from the young men was intended for oral administration. Inasmuch as the boys died and the Pope received no benefit, it seems fruitless to argue the merits of the various versions of this account. Hieronymus Cardanus (1505–76), Magnus Pegelius (1593), Andreas Libavius (1615), and Giovanni de Colle (1628) seem to have considered the possibility of transferring blood directly from the vessels of one animal to those of another, but the claimants for the credit of first successfully accomplishing this operation are usually listed as Richard Lower, Francesco Folli, Jean Baptiste Denys, Johan Daniel Major, Robert des Gabets, and Tardi. As one may infer from the names, nearly every European nation has its aspirant for this honor. Disregarding the contending claims for priority of idea, priority of accomplishment, and priority of publication, it is certain that transfusion of blood

from animal to animal and from animal to man was done repeatedly by various workers during the latter half of the seventeenth century. Because of difficulties, deaths, and doubtful utility, it was abandoned until the nineteenth century, when Blundell (who advocated the transfusion of blood from man to man) revived it. The subsequent development of blood transfusion is not a part of this story, although some reference to it will be made in another section.

Not credited by all historians as valid, an early motive for resorting to intravenous injection was the morbid curiosity of one Hans Gurge of Ehrendorff, riding and kennel master to an Austrian nobleman, who is reported by Ettmüller to have injected Spanish wine into the veins of dogs in 1642 for the purpose of making them drunk, their inebriated actions and howlings providing amusement to the owner. It is pertinent to this account, whether valid or not, to note that a person with a depraved sense of humor, wishing to administer wine to an uncooperating dog in 1642, had no alternative to the intravenous route, for the apparatus and technique of administration by stomach tube were not yet developed, and wine is much too irritant a clyster to remain long enough in the short colon of a dog to permit much absorption.

Quite generally accepted as being the first experiments involving the intravenous injection of drugs, the experiments of Sir Christopher Wren and his collaborators may also be credited with being designed for serious scientific purposes. Henry Oldenburg, editor of *Philosophical Transactions* (Vol. 1, 1665), in "An account of the Rise and Attempts of a way to conveigh Liquors immediately into the Mass of Blood" states:

> 'Tis notorious, that at least six years since (a good while before it was heard of, that any one did pretend to have so much as thought of it) the Learned and Ingenious Dr. Christopher Wren did propose in the University of Oxford (where he is now the worthy Savilian Professor of Astronomy, and where very many Curious Persons are ready to attest this relation) to that Noble Benefactor to Experimental Philosophy, Mr. Robert Boyle, Dr. Wilkins, and other Deserving Persons, that he thought he could easily contrive a Way to conveigh any liquid thing immediately into the Mass of Blood.

And, according to his report, experiments to carry out these suggestions were promptly instituted.

This somewhat belated claim for Wren's priority was occasioned by the appearance of Johan Daniel Major's "Chirurgiae Infusiorae" in 1664, and John Elsholz' "Clysmatica Nova" in 1665, and the reports that Carolo Fracassato had performed similar experiments in Pisa in 1658 likewise prompted Timothy Clarke in a later communication to the *Philosophical Transactions* to correct Oldenburg's date (1659?) and fix it definitely as 1656.

These experiments, in which opium and crocus metallorum (sulphuret of antimony) were injected into the veins of dogs, established the fact that these drugs produced the same effects (stupefaction and vomiting, respectively) as they did when administered by mouth, and thus dealt a mortal blow to the prevailing doctrine of sympathy, which held that drugs produce their characteristic effects by acting upon specific loci in the gastrointestinal tract, thereby inducing reflex effects in those organs sympathetically related to these sites. These may, therefore, be called the first experiments in pharmacology directed toward the analysis of the mechanism of the action of drugs.

The experiments with intravenous injection by Major, Elsholz, Scotus, Hoffman, Fabricius, Schmidt, Ettmüller, and others, which followed closely after those of the Oxford group (Wren, Boyle, Wilkins, Lower, Clarke), may be regarded as in the main motivated by the desire to determine whether drugs would have the same curative effects when administered in this fashion as when given by mouth. Various of these early investigators commented upon the fact that their results did or did not indicate that drugs had to be modified by the gastrointestinal secretions in order to be effective, that the method enabled physicians to give drugs to unconscious or uncooperative patients, and that the promptness of effects

might be of advantage in certain instances, etc.; but, generally speaking, all the seventeenth-century experiments may be viewed as efforts to appraise the intravenous route as merely an alternative to oral medication and of only theoretical interest in view of the extreme hazard. As might be expected, miraculous and incredible cures were claimed by some and denied by others. But, plagued by frequent and severe, and often fatal, reactions, the practice lapsed for about a century.

Thus it was that in 1821, when E. Hale wrote his Boylston prize essay, "On the Propriety of Administering Medicine by Injection into the Veins," he felt called upon to survey the necessities and advantages of the method as follows:

> The extravagance of the effects imputed to the injections so entirely destroys the credibility of the accounts, that no reliance can be placed in them, except to show that the attempt has long since been made to take advantage of this mode of administering medicine. In every other point of view we must regard this as a new question, at least so far as the practicability of the injections is to be determined.

Hale reduced the motives or indications to two: to remove a mechanical obstruction of the esophagus by injecting an emetic intravenously, and to give a cathartic intravenously when irritability of the stomach precluded its oral administration. In view of the frequent mention of the former indication by other writers as well, it seems that our ancestors of this era were often guilty of bolting their food in such a manner as to render esophageal obstruction of this sort a not-infrequent episode.

With a greatly renewed interest in the feasibility of intravenous injections in the nineteenth century, we find an equally expanding variety of motives. Magendie, seeing the piteous victims of hydrophobia, unable to swallow and often thrown into convulsions at the mere suggestion of swallowing that the sight of water provoked, advocated the surreptitious intravenous injection of water in such cases. Thomas Latla, noting the marked loss of water and salt in cholera victims, suggested the intravenous injection of saline solutions as a replacement measure; and various workers, noting the evident starvation effects in patients with intractable vomiting, made attempts at intravenous feeding, using milk, egg white, and sugar.

In 1841 James Blake injected baryta into the jugular vein of dogs and watched for its appearance in the opposite carotid as a means of determining the circulation time in animals. This agent, detectable chemically, was thus the forerunner of a host of agents detectable in a variety of ways (dyes—colorimetrically; salts—by electrical conductivity changes; physiologically active substances—by specific symptoms; radioactive agents—by detection of radioactivity) that have been used for this purpose since. Intravenous injections were employed by the experimentalists of this period, for, as Blake states:

> In my experiments I have always investigated the physiological action of a substance by introducing it directly into the veins or arteries of a living animal. It is thus with the smallest disturbance possible to the normal condition of the animal, brought into direct contact with the tissues whose reactions we wish to investigate. Its subcutaneous injection or introduction into the stomach gives rise to complications which are better avoided.

To come back to clinical motives of the nineteenth century, cold water was injected into the umbilical vein to facilitate separation of the placenta, and, noting that absorption of drugs was often slow in patients with pronounced edema, practitioners proposed to produce an artificial plethora by the injection of tepid water into the veins to delay absorption in cases of drug poisoning.

The recognition that a characteristic feature of the intravenous injection of drugs was promptness in the exhibition of the full effects of the dose administered and a corresponding promptness in recovery owing to rapid elimination brought about the attempt to use the intravenous route for the administration of anesthetics in the nineteenth century with the agents then available (ether, chloroform, chloral hydrate), because these attributes were recognized as desirable features of anesthesia. It was not, however, until the discovery of the agents that became available

in the twentieth century that intravenous anesthesia became a feasible and successful procedure.

Toward the close of the nineteenth century came the recognition of the bacterial causation of many diseases, the knowledge that in many of these cases bacteria are present in the blood stream, and that many chemical agents are more or less destructive of bacteria. Countless attempts to cure infectious diseases by the intravenous injecton of phenol, mercury and silver salts, and all sorts of antibacterial agents promptly followed. These attempts were so numerous, so extensive, and so thoroughly planned that their futility and lack of promise were disappointing and discouraging in the extreme. Von Behring gloomily concluded:

> It can be regarded almost as a law that the tissue cells of man and animal are many times more susceptible to the poisonous effects of disinfectants than any bacteria known at present. Therefore, before the antiseptic has a chance either to kill or to inhibit the growth of the bacteria in the blood or in the organs of the body, the infected animal itself will be killed. The pessimism of him who declared that disinfection in the living body is for all time impossible appears to be only too justified.

WITH the twentieth century new indications and motives, as well as extensions and embellishments of older motives, appeared. Although suggested long since and sporadically employed, the intravenous injection of various saline solutions did not come into general use until World War I. Improvements and development of the technique of blood transfusion had taken place, but not to the extent that it was promptly applicable in cases of shock and hemorrhage. Thus the search for blood substitutes, with which the circulating blood volume could be restored, was extremely vigorous, and much of our current knowledge of the injectability of various things had its origin in these war-inspired researches. The use of saline solution to replace lost fluid and lost electrolytes, to correct disturbances in the acid-base balances of the blood, etc., came out of these studies. So, too, did recognition of the fact that the protein level of the blood plasma is important in the regulation of the water deposits in the body and that the intravenous injection of plasma proteins, or substitutes, such as acacia, would relieve cases of edema due to hypoproteinemia.

The recognition that certain substances are eliminated exclusively by the kidneys in the urine or by the liver in the bile resulted in the development of methods for determining the functional capacity of these organs, involving the intravenous injection of readily detectable substances. Rendering the injected agents radiopaque provided means whereby the urinary or biliary tract could be outlined radiographically for diagnostic purposes.

Shortly after the war came the use of intravenously injected hypertonic solutions (first, sodium chloride; later, dextrose and sucrose) to reduce cerebrospinal fluid pressure by osmotically dehydrating the brain and ventriculo-subarachnoid system when, in cases of concussion, skull fracture, or brain tumor, the indications for doing so were present.

Injection of radiopaque materials to outline blood vessels and of fluorescent material to determine the patency and competency of the vascular supply where signs of gangrene or vascular obstruction warn of the necessity of surgical intervention are some of the more recent motives for this procedure.

The methods employed do not need to be described at length. Whether we place any credence in the legend recounted by Pliny that the ancients learned of the potential uses of the enema from the Ibis, which agile bird, according to this account, "washes the inside of his body by introducing water with his beak into a channel by which our health demands that the residue of our food should leave," the fact remains that the clyster art was highly developed, and there were many types of apparatus adaptable to the procedure of intravenous injection when this latter was first thought of. And so we find the earliest experimenters using quills, hollowed-out chicken bones, silver tubes, etc., as the instruments to insert in the sectioned vein, and compressible bags (the scrotum or bladder

from various animals, silk purses, etc.) or piston-type syringes or, on occasion, the mouth of the operator as the container of, and propelling mechanism for, the injected material. The designation of the intravenous injection procedure as *Ars Clysmatica Nova* or *Chirūrgia Infusoria* gives testimony to the lowly origin of the techniques employed, and we find most of the early workers using the expression that such and such a substance was "thrown into" the blood.

The sharp-pointed, detachable syringe needle and syringes, which may properly be called the precursors of the conventional all-glass syringe of today, evolved about the middle of the nineteenth century, owing to the labors of Rynd, Thomas Wood, and Pravaz. Thus it is that intravenous injections antedated subcutaneous injections by hundreds of years, and also that surgical exposure of the vein to be injected, with its accompanying hazard of infection and phlebitis, was necessary until these more modern instruments had been perfected. Learning that it did not take great force to impel fluids into the veins, various types of gravity infusion apparatus were also developed. These, too, have their current counterparts in everyday use.

The difficulties, problems, and reactions encountered throughout the development and perfection of the intravenous technique were numerous and disconcerting. Those peculiar to blood transfusion need not be recounted fully here, as the tale is formidable in itself and has been dealt with extensively by numerous writers. Suffice it to say that in 1667 one of the pioneers noted that "transfusing a great quantity of blood into several Doggs, observed alwayes, that the Receiving Doggs pissed Blood" and that, although homologous transfusions were advocated by some as early as 1818, it was not until 1882 that Cohnheim, upon the basis of his own studies and those of Creite, Landois, and others, concluded that "the serum of one species is a direct poison to the corpuscles of another"; not until 1900 that Landsteiner added the fact that there could be incompatibility of blood within a species because of isoagglutinins, not until 1914 that the problem of keeping blood incoagulable as a means of facilitating its administration was solved; and not until more recent years that the problems of the Rh factor, etc., were understood. And—it may be added—the end is not yet.

As to the tribulations involved in intravenous injections in general, Oldenburg in his 1665 account (of Wren's work) gave hint of what might be anticipated as follows: "Some whereof, though they may conceive, that Liquors thus injected into the Veins without preparation and digestion, will make odde commotions in the Blood, disturb Nature, and cause strange Symptoms in the Body." Major had noted (1664) that the injection of acids coagulated the blood and thereby caused death promptly. These observations were quickly corroborated by Fracassatus and others, who noted, in addition, that the animals appeared to die of suffocation and at post-mortem showed a characteristic frothy, slimy appearance of the lungs. Although it was a long time before this accident (of pulmonary embolism due to thrombosis) was clearly defined and its hazards appreciated, it cannot be doubted that it was a frequent cause of the severe reactions and fatalities with which this procedure had to contend, and that in a sense it was the first hazard peculiar to intravenous injections which was partially resolved and understood.

Although septicemia and pyemia as well as phlebitis were frequently encountered and commented upon by the early pioneers, these had been experienced as hazards attending bloodletting procedures alone, so that they were not construed as primarily related to intravenous injections per se. Accordingly, it may be said that air embolism was the next hazard associated with the procedure to be recognized. There can be little doubt, in view of the techniques of 1664, that it was air embolism (owing to the aspiration of air through the gap in the vein because of the negative pressure incident to respiration) that explained Major's findings that he could inject into the leg veins of dogs without harm many things which caused more or less instant death when he attempted to inject them into the jugular vein. But Major did

no more than comment on this strange paradox. According to Fortescue-Brickdale, it was Friend, whose complete works were published in 1773 after his death, who was the first to demonstrate the danger of the entry of a large quantity of air into a vein and the first to note the characteristic frothy distension of the right ventricle at post-mortem after such an accident.

That the serious consequences occasioned by the accidental injection of air are brought about by the ability of air bubbles to act as emboli in obstructing the pulmonary circulation was not recognized for some time. Many workers presumed that it aroused a peculiar excitability of the coats of the veins and therefore argued that, if such a bland, nonirritating substance as air could do this with fatal consequences, it would be unlikely that anything save blood itself could ever be used successfully. Hale, who gave voice to such sentiment, concluded, however, that it was fruitless to explore the proposition on theoretical grounds, or even to determine it by animal experiment alone, and thus arrived at the decision to make his tests upon himself. His account of the bold experiment in which, in spite of great difficulties, he injected one ounce of caster oil into his arm vein, entitles him to a place on the honor roll of martyrs in medicine, for though he survived the experience and his arm was not permanently ruined, his good luck in surviving was such that, as Fortescue-Brickdale put it, "It seems surprising that circumstances did not compel him to limit his protest to the length of an epitaph." It was not until some years after Hale's experiments that authorities concluded "that oils, injected into the veins interrupt the circulation and produce a kind of asphyxia." Fat embolism, as we now call it, was thus another variation in this type of hazard that came to be recognized.

In 1841 Rees showed that—

> agents which alter the specific gravity of the serum of the blood exert an influence on the blood corpuscles by affecting the endosmotic currents through their membranes; agents which increase the specific gravity of the serum collapse the corpuscles, while those which lessen the specific gravity distend them. Water injected into the jugular vein of the dog caused rapid distension of the corpuscles and rupture of their membranes.

Crenation of corpuscles, on the one hand, and hemolysis, on the other, thus became recognized, and the desirability of injecting materials in isotonic concentration was established. Or, in other words, it came to be realized that substances intended for intravenous injection should have their miscibility with blood determined.

Nausea and vomiting, chills and fever, have dogged the steps of intravenous injection throughout its history. In a letter translated by Oldenburg and appearing in the *Philosophical Transactions* of 1667, Fabricius, commenting upon the results of the injection of "a Laxative Rosin, dissolved in an Anti-epileptical Spirit" into three patients, states: " 'Tis remarkable, that it was common to all three to vomit soon after the injection, and that extremely and frequently; the reason whereof we leave to intelligent Physicians to assign." Since the drug used produced only catharsis when administered orally, it seems capricious of Fabricius to veil the explanation in this whimsical fashion, for even now, nearly three hundred years later, although nausea and vomiting can be attributed to distinct causes in many instances (e.g., it is part of the symptom complex produced by embolic, pyrogenic, and anaphylactic reactions, etc.), there remain cases in which it seems to have a mysterious psychogenic origin, occurring in patients after a first injection, but disappearing with succeeding injections.

With the advent of the science of bacteriology, it seemed plausible that all instances of chills and fever consequent to intravenous injection could be attributed to the unavoidable accompanying injection of contaminating bacteria. But, alas, asepsis and antisepsis failed to prevent febrile reactions, and it was not until 1923 that Florence Seibert showed that bacteria, growing in more favorable media or even in distilled water, produced a filterable substance (stable to boiling, the usual sterilizing procedure) called pyrogen, which produced chills and fever when in-

jected intravenously; and it was not until about ten years later that a reasonably successful elimination of pyrogen from injection materials was accomplished.

The hazard of anaphylactic reactions was only tardily appreciated, as might be expected in view of our current knowledge. That certain substances (chiefly proteins, but not all proteins) could be innocuous upon a given injection and then markedly poisonous upon a subsequent injection (if the latter were given at an appropriate time interval) was a startling and apparently incredible observation. But, securely established as a fact, it became the starting point for a tremendous effort at its understanding and, more pertinent to the present story, the established practice of studying the reinjectability of everything that was considered as meriting injection in the first place. It has been learned that the agents that may provoke anaphylactic reactions are those that engender the formation of specific antibodies; hence, they are called antigens, and agents to be injected are now tested for their antigenicity. In view of the relative seriousness of anaphylactic reactions, this question is of paramount importance.

Perhaps the most recent claim to the role of a hazard peculiar to intravenous injection is that of the untoward effects associated with rapid administration, which has been given the name of "speed shock." Noting that various substances, innocuous upon slow injection, produced marked fall of blood pressure, respiratory irregularity, etc., upon rapid injection, investigators postulated that some of the injected molecules, speeding rapidly to the liver and doing damage to the liver cells, thereby caused the observed effects. The studies of DeBakey, Milbert and Rothwell and Crocker, and others have shown that there is no rational basis for this conception, but it still remains a fact that the speed of injection is of the utmost importance with several kinds of agents. For example, the rapid injection of a soluble barbiturate salt will cause respiratory arrest before enough has been injected to produce anesthesia, and the rapid injection of a soluble quinine salt will cause cardiac embarrassment and fall of blood pressure in a dose that is perfectly safe if slowly administered. Similar experiences obtain with bile salts, potassium salts, ether and chloroform, etc. It is quite clear that these are concentration effects occurring with substances which diffuse readily throughout the body so that, when given slowly, they produce their characteristic pharmacological effect when the total body concentration of the drug is at the requisite level, but when injected rapidly there may be a very high concentration of the drug carried in the blood to the susceptible tissues. It is also clear that for drugs like arsphenamine, which are poorly soluble at the pH of the blood, there will be some drug coming out of solution and producing the effects of particulate matter (embolism, etc.) when injected rapidly—effects that can be avoided by slow injection. Thus, there is some validity in the idea that there is a "speed-shock" factor in connection with certain intravenous injections, without drawing upon some mysterious velocity factor that should obtain for all.

It seems unnecessary to add that the total volume of fluid to be injected is now also recognized as a potential source of strain to the heart and circulation.

Thus, we find the progress of intravenous therapy an oft-repeated series of interruptions. Enthusiastically pursued by the pioneers of the seventeenth century, Wren, Boyle and Company, Major, Elsholz, Fabricius, Fracassatus, and others, the method was soon abandoned, voluntarily because of its difficulities and involuntarily in many places by virtue of the misgivings of both civil and professional authorities. Revived in the 1770s by Liberkuhn and Laesecke and others, it lapsed once more; revived in 1814 by Baron Percy and others, it declined again; revived in the 1840s by the experimentalists, Bichat, Blake, Magendie, Bernard, and others, it receded again; revived by Landerer in the 1880s, it had its last decline, largely because of the disappointments associated with the early efforts at chemotherapy. It was revived finally by Ehrlich (in 1909), whose signal success with Salvarsan in the treatment of syphilis

and whose convincing demonstration that this drug could not be given effectively in any other way, placed intravenous therapy on the firm ground it has held ever since. It is almost true that each interruption and each renewal of effort marked the recognition of a hazard, on the one hand, and a partial solution to that problem, on the other.

FINALLY, let us consider the lessons learned indirectly from the prosecution of this procedure. It is a virtual impossibility to give the correct genealogy of facts, and so it is likely that in this account I shall be guilty of attributing both too much and too little to this endeavor. It would seem indisputable, however, that we have learned what we know about the desired characteristics a substance intended for injection should have as the direct result of making such injections: that is, it should be sterile and hence either sterilizable or available in sterile form, as is blood; it should contain particulate matter not at all, or at least not as large in size as the blood cells; as a corollary of this it should be soluble in water and in blood and should give rise to no particulate aggregates by agglutinating blood cells or precipitating plasma proteins; it should be isotonic, or nearly so, and without specific hemolytic effect upon the red blood cells; it should be nonirritating, or nearly so, to the vascular endothelium; it should have approximately the same pH as that of the blood, or the quantity to be injected must remain quite small; it should be free of pyrogen; and it should be nonantigenic. Many drugs that are injected intravenously as a routine matter do not meet all these criteria, some because we have no alternative, and some because the violation is intentional (e.g., hypertonic solutions as used for raised intracranial pressure). The organism has considerable tolerance for some departure from the ideal, particularly if care is exercised and undue haste avoided.

A large part of what we know about the blood arose directly from the efforts to use it as a material for venous injection. The vast scope of the subject of hematology precludes any recital of this achievement. A number of curious pharmacological facts were observed as the direct result of this procedure: for example, that drugs may have quite dissimilar effects in different species of animals (e.g., morphine was observed as depressant to dogs and stimulating to cats by the first pioneers); and that, although a number of drugs (snake venoms and curare, in particular) could be innocuous by mouth and deadly poisonous by vein, this was by no means a universal tendency, for cherry laurel was found to be innocuous by vein, although highly poisonous by mouth. As indicated earlier, the first intravenous injections of drugs were made for the purpose of solving, at least in part, the mechanism of the action of drugs, and such injections have been indispensable in later studies of this problem.

Intravenous injections led to the recognition of pulmonary embolism as a pathologic entity, as well as to that of air and oil embolism and embolic phenomena in general.

As a by-product of the attempt at intravenous feeding, Schmidt-Muhlheim observed in 1882 that the split products of harmless proteins may be highly toxic (producing what is called peptone shock). This observation, focusing attention upon the necessity of learning the nature of the ultimate nutrients to which the processes of digestion reduce our foods, may be considered in many respects the cornerstone of the subject of nutrition.

The observations regarding anaphylaxis, made incidental to the prosecution of this technique, are a similar cornerstone to the subject of allergy. The recital is by no means complete, but it seems enough to warrant the statement that seeking a new way to do something may lead to many and unpredictable adventures.

THE NATIONAL FERTILIZER ASSOCIATION

CHARLES J. BRAND

Mr. Brand (B.S., Minnesota, 1902) was born in 1879 on a farm in Lac qui Parle County, Minnesota. After an aggregate of twenty years in research, administrative, and consultative capacities in the U.S.D.A., he became executive secretary-treasurer of the NFA, a position from which he has only recently retired to work as an economic consultant on agricultural and industrial problems. In 1946 he founded at the University of Minnesota the Conway MacMillan Research fellowship in botany, honoring a distinguished scientist and beloved teacher.

THE great functions of the commercial plant-food industry are the preservation, restoration, and improvement of the nation's soils, increased production of food, feed, fiber, and other crop plants, and improvement of their quality. The industry encourages every good farming practice relating to soils and crops. Among these are use of good seed; growing desirable varieties; saving and effective use of farm manures and crop wastes; good tillage; crop rotations, including seeding of clovers useful in supplying nitrogen; plowing under green manure and summer cover crops; use of lime when needed; terracing and contour farming to reduce erosion; and use of insecticides and fungicides.

Too little is known by the public and farmers about the removal of plant food by crops and erosion. Few people realize the devastation of the soil that results from continual maximum crops. In 1946 the nation was blessed beyond anything that could be expected—and this on top of five or six prior years of bountiful harvests. We had crops of 3,380,000,000 bushels of corn, 1,170,000,000 bushels of wheat, 2,500,000,000 pounds of tobacco, and 478,000,000 bushels of potatoes. These, plus cotton, used about 70 percent of all the fertilizer consumed in the United States. Fruits and vegetables, oats, hay (including alfalfa), and sugar crops consumed most of the other 30 percent. Table 1 shows the draft certain crops make upon soil fertility.

The principal elements on which the present chemical fertilizer industry is based are nitrogen, phosphorus, and potassium. Other soil elements are important, but these are regarded as the major plant-food elements. Nitrogen is expressed as pure N (formerly it was expressed as ammonia); phosphorus, as phosphoric acid (P_2O_5); and potassium, as potash (K_2O).

Nitrogen materials include, among others, nitrate of soda, sulphate of ammonia, ammonia solutions, ammonium nitrate, cyanamide, urea, and, to a minor extent, such organic materials as tankages and seed meals.

Phosphorus in the United States is obtained almost exclusively from superphosphate, made by treating phosphate rock with sulphuric acid. There is also a minor use, tonnage-wise, of ammonium phospate, ground phosphate rock, bone meal, and basic slag. In western Europe basic slag is almost as important a source of phosphorus as phosphate rock. Before World War II, Germany alone produced more than 5,000,000 tons of basic slag, of which she used approximately 2,500,000 tons in agriculture, exporting the remainder to other countries.

Potassium is obtained chiefly from muriate of potash (potassium chloride), sulphate of potash, sulphate of potash-magnesia, and manure salts.

Secondary elements—calcium, magnesium, and sulphur—and minor elements—iron, boron, manganese, iodine, zinc, copper, and still others—may be required in some areas and for some crops. Carbon, hydrogen, and oxygen are also essential and are obtained from air and water. These three usually account for about 90 percent of the total weight of plants.

In the United States the chief use of com-

mercial fertilizers is in the form of mixtures, or complete fertilizers, consisting of suitable compatible materials containing the three primary plant-food elements. Materials are described and sold by name and plant-food content; e.g., muriate of potash, 60–62 percent. Mixed or compound fertilizers are described by a series of three numerals, such as 5–10–5, 8–16–8, 5–8–7, 0–10–10. The 5–10–5 grade contains 5 units of nitrogen, 10 of phosphoric acid, and 5 of potash. The 0–10–10 grade contains no nitrogen. The unit is 20 pounds, or 1 percent of a ton. The effectiveness of fertilizers is frequently related to the proportional amount of each plant food present in a mixture.

TABLE 1

PLANT FOOD REMOVED BY CROPS*

Crop	Yield Basis	Pounds per Acre			
		Nitrogen	Phosphoric Acid	Potash	Total
Cotton	250 lbs of lint, plus seed	18.7	7.3	7.5	33.5
Tobacco	900 lbs. of leaf	24.5	3.9	35.5	63.9
Wheat	15 bu., plus straw	29.6	19.2	27.9	76.7
Corn	30 bu., plus stover	47.2	20.8	42.8	110.8
Alfalfa	2 tons	94.0	21.9	89.0	204.9

* Per acre of stated yields.

Early organizations in the fertilizer industry. In 1860 there were 47 fertilizer manufacturers. The value of the finished product was $891,000. (It is estimated that in 1946 there were 800–900 manufacturers and that a total of 14,000,000 tons of commercial fertilizers and fertilizer materials, valued at the farm at $440,000,000, were produced.)

The first commercial fertilizer to enter world-wide commerce was guano, obtained chiefly from the islands off the west coast of South America. Consuming countries began importations in quantity in 1841. By 1852 no less than 20 countries were exporting guanos of widely variable plant-food content. Great Britain and the United States were the chief consuming nations. Great Britain imported 283,300 tons in 1844. United States imports rose from 1,000 tons in 1847–48 to 175,800 tons in 1853–54, of which 164,000 tons came from Peru.[1] The demand for guano was so insatiable that the best deposits were largely exhausted in three or four decades.

Trade and maritime conditions, state and municipal laws and regulations, tax problems, and other factors resulted in the organizing of "fertilizer exchanges" in the chief receiving ports, particularly Baltimore, New York, Philadelphia, Charleston, and Savannah. The utility of these exchanges grew as the list of fertilizer materials and compound fertilizers grew. By 1876 this list included nitrate of soda, phosphates, brimstone for making sulphuric acid, kainite, muriate of potash, and some organic materials like bones, fish, and tankage.

There were many common problems, especially those related to lack of uniformity in chemical analyses and fertilizer control by the states. As a result, the National Association of Chemical Fertilizer Manufacturers was organized in 1876 in Baltimore. This was the first comprehensive organization of which there is record. It met regularly for several years to consider "matters upon which our interests cease to be divided and become common" and after a discontinuous existence, became inactive.

On May 31, 1883, in the rooms of the Chemical and Fertilizer Exchange of Baltimore, the constitution of the first body, known as The National Fertilizer Association, was

adopted. This organization was the successor of the one formed in 1876. The annual meeting for election of officers was held August 29, 1883, and three interesting addresses were made. One dealt particularly with the problem of chemical analysis, which continues to be a subject of active work even to the present day. Committees on transportation, trade rules, arbitration, state laws, agricultural chemistry, and so forth were appointed.

The new organization functioned until 1887. From 1888 to 1893 no general organization existed. In March 1894, at Columbus, Ohio, the forerunner of the present National Fertilizer Association, known as The Association of Fertilizer Manufacturers in the West, was organized. Its first annual convention was held at Buffalo, New York, November 20, 1894. At its eighth annual convention in June 1901, its name was changed to the Fertilizer Manufacturers Association. At the fourteenth annual convention, held during the Jamestown Exposition at Norfolk, Virginia, in October 1907, the name was changed back to The National Fertilizer Association, as it had been from 1883 to 1887.

If the old National Fertilizer Association that held its first meeting in 1883 had been continued, it would now be nearly sixty-five years old. The present organization, and its immediate antecedents dating from 1894, have been in existence nearly fifty-four years, a long period in trade-association history.

In June 1906 there had been organized at Old Point Comfort, Virginia, a regional association called The Southern Fertilizer Association. The National and the Southern, with a number of members paying substantial dues to both, continued their separate existences until June 1925. Then, after several months of negotiation, they were merged into a truly national body bearing the name of the The National Fertilizer Association.

This Association (hereinafter, with its predecessors, referred to as the NFA), was a voluntary, unincorporated body. Organization was perfected at White Sulphur Springs, West Virginia, in June 1925. On June 11 it held its first meeting, adopting bylaws, electing officers, and holding a brief program session. Its initial active membership of 145 companies covered the production of about 80 percent of the total annual tonnage of the industry, including superphosphate and compound fertilizers. By June 1926 there were 168 active members and 74 associates.

The bylaws provided for dividing the fertilizer-using territory, excluding the Pacific Coast states, into nine districts for the purpose of electing an executive committee, which was constituted the governing body. One member was elected to represent each district, and nine were elected at large at the annual convention. To assure democratic management, six members were from small, six from medium-sized, and six from large companies.

On June 1, 1927, the NFA was changed from a voluntary association to a corporation under the laws of Maryland. This action was taken in consequence of an antitrust action which did not involve the Association but did involve a number of its members. New bylaws were submitted to the Department of Justice to assure as far as possible procedures that would not be regarded as in themselves likely to run counter to the antitrust laws.[2] The former Executive Committee was discontinued, and a Board of Directors was established, with an authorized maximum of 21 members.

In 1927 California was constituted the tenth district, and on January 1, 1928, all manufacturing members of the California Fertilizer Association became affiliated with the NFA. Subsequently, Weller Noble, chairman of the Soil Improvement Committee of the California Association, was elected to the Board of Directors of NFA and made chairman of the Pacific Coast district.

This situation continued until the period of the National Recovery Administration in 1933–35, when the whole country was divided into 12 districts and the number of directors was increased to not less than 33. The Board is the policy-making agency, with absolute control of the purse strings, subject to a vote on the annual budget by the mem-

bers in convention assembled. An executive secretary–treasurer was elected to administer the affairs of the Association under the Board's supervision.

Fertilizer-control legislation. Prior to 1869, no recorded attempt was made to institute any statutory control over importation, manufacture, and sale of fertilizer. For twenty-five years hundreds of cargoes of guano had been imported with no determination of their fertilizing value. Farmers and distributors had soon noticed that one cargo produced crop increases decidedly different from another cargo, despite the fact that physical appearance and price were the same. This led to so-called manipulated guanos, really mixed fertilizers, prepared by fortifying and balancing the natural product. Agricultural workers and the growing fertilizer industry were aware of the undesirable situation, and in 1869 the first fertilizer-control law was adopted in Massachusetts. Dr. Charles A. Goessman, then chemist of the Massachusetts Agricultural College at Amherst, was largely responsible for the adoption of this law. After receiving his training in agricultural chemistry in the University of Göttingen, he had come to the United States in 1857. Today, 47 of the 48 states have fertilizer laws. Only Nevada lacks one. In 1873 the Massachusetts law was revised because of its faulty character. Later, when fertilizer organizations had come into existence, many states passed new laws and revised old ones, often in line with the experience and recommendations of Association members.

A single case will illustrate the need of uniformity in chemical control. Potash salts began to come from Germany in the 1860s. By 1883 consumption of German potash in the United States approximated 25,000 tons. The technique of chemical analysis was so undeveloped that four reputedly experienced chemists reported the content of potash in a single fertilizer sample as follows: 0.50, 0.51, 1.28, and 1.65. Two chemists, evidently using too much heat, failed to find any potash at all on their first run. Experiences like this caused the NFA in the early 1880s to cooperate with the agricultural interests in the state legislatures to prepare a draft of a uniform law regulating fertilizer-control work. The officers of the NFA and of the Chemical and Fertilizer Exchange of Baltimore, particularly A. de Ghequier, then secretary of both, aided in drafting state legislation. In 1884 de Ghequier also assisted in organizing the Association of Official Agricultural Chemists (AOAC) at Philadelphia. He participated in the principal preorganization meeting in Atlanta in 1883 and was a member in its early years.

Baltimore is recognized as the cradle of the American chemical fertilizer industry. It has been the leading center of production since before 1850. Philadelphia was also an early manufacturing center. One of its first producing concerns, organized by John P. Baugh in 1855, operates today at Baltimore under the name of The Baugh and Sons Company. Another pioneer was G. Ober & Sons Company, of Baltimore, established in 1857. In later years it was acquired by The Davison Chemical Corporation, also one of the oldest in the industry, originally operating under the name of Wm. Davison & Company.

Before closing our comments on the early history of fertilizer legislation, it should be said that the AOAC, state chemists, commissioners of agriculture, and directors of experiment stations endorsed the NFA's efforts to obtain uniform state legislation. Dr. Harvey W. Wiley, for many years chief of the Bureau of Chemistry of the U. S. Department of Agriculture, and celebrated leader in pure-food legislation in America, was also secretary of the AOAC for many years. In an address at the annual convention of the NFA at Norfolk, Virginia, in October 1907, he stated with reference to fertilizer laws, definitions, methods of analysis, labeling, etc.: "In all these matters it is highly desirable that there should be unity of action by The National Fertilizer Association and the Association of Official Agricultural Chemists."

When the present NFA was organized,

one of its earliest activities in 1925 and 1926 was to redraft the model state fertilizer law in the light of all the state laws then in effect, and to assist states in adapting it to their individual needs. Work of this kind apparently never ends. The lead article in the November 2, 1946, issue of the *American Fertilizer* (Philadelphia: Ware Bros., **105**) was entitled "Proposed Uniform State Fertilizer Bill," with the subheading "Control Officials Association prepares measure; plant food content set at 20 per cent minimum; official list of grades included."

The annual volume entitled *The Fertilizer Movement During the Season 1885–86,* published by the NFA in August 1886, contained a full reprint covering 167 pages of the laws relating to the sale and inspection of fertilizers and the rules and regulations established under these laws. The laws of 24 states were reproduced. The NFA's interest in constructive legislation still continues. Every other year it prepares and distributes a detailed tabular analysis of all state fertilizer laws.

Cooperation between NFA and AOAC. This activity goes back to 1884, when the AOAC was organized. As shown by Dr. Carl L. Alsberg's Preface to the first (1920) edition of *Official and Tentative Methods of Analysis,*[3] it was organized by men well known in the field of chemical control of industrial chemicals, foods and feeds, and other products. A few of them, like A. de Ghequier, were not government officials. Later, as members were engaged almost exclusively in law-enforcement work, membership was and now is confined to representatives of governmental bodies.

With the continuing growth of the fertilizer industry and the increasing perplexity of its chemical control problems, the AOAC set up a special Committee on Definition of Terms and Interpretation of Results on Fertilizers. New developments were constantly taking place, such as manufacture of concentrated superphosphate, 43–48 percent P_2O_5 (1907, United States); air fixation of nitrogen by the cyanamide (1898–1901, Germany) and direct synthesis (1905–13, Germany) processes; production of more concentrated potash carriers; and ammoniation of superphosphate (1928–31, United States). The NFA cooperated constantly with the AOAC, making its representations from the industry standpoint, so that the AOAC rules and procedures would conform to the needs of the industry and the farmer.

Promoting uniform terminology and practices. Only a listing of some of the activities for promoting uniform terminology and practices is possible. They include—

1. Use of the term "nitrogen" instead of the term "ammonia."
2. Expressing fertilizer grades or analyses in even units.
3. Stating the plant foods in order of N, P, and K.
4. Use of the term "superphosphate" instead of "acid phosphate."
5. Including grade numerals as part of the brand name.
6. Increasing the minimum plant-food content in mixed fertilizers and superphosphate.
7. Reduction in the number of analyses offered for sale.
8. Simplification of guarantees and of printing on bags and tags.
9. Proscription of cutting of materials.

Space precludes extended discussion, but a few comments may be of interest. Formerly nitrogen was expressed in terms of ammonia, although few nitrogen carriers contained N in the ammonia form. As chemical analysis cannot determine with certainty how much of the nitrogen in a specific fertilizer is obtained from each different nitrogen source, an illustration of diversity in prices is illuminating. Cyanamide became freely available in 1910, the American Cyanamid Company having been organized in 1907. The nitrogen contained therein behaves the same as organic nitrogen (for example, in fish), but at that time a unit of 20 pounds of cyanamide nitrogen was valued at $1.00, whereas a unit of organic nitrogen, termed "organic ammonia," from fish, was valued at $3.00, and the unit value in cottonseed meal was $2.50.

In recent years unfriendly critics in and out of Congress have said much about the alleged "dirt," and "worthless," or "inert"

material that manufacturers are accused of selling to the farmer. There is little merit in the accusations since farmers owning cooperative companies sell pretty much the same mixed fertilizers as do the individually owned and corporate units of the industry.

In earlier years there was more basis for complaint. Even twenty-five or thirty years ago some mixed fertilizers contained as little as 10 or 12 units of plant food. Grades such as 1–8–1, 2–6–2, and 2–8–2 were not unknown. The terms "low-grade" and "low-analysis" were then to some extent applicable to such goods, but this condition was partly a result of the low plant-food content of the materials then available.

When average plant-food content was probably about 14 percent, the board of directors of the NFA instructed its officers and staff to take every proper means to promote raising the minimum content first to 16 and then to 18 percent. If the content had remained at the 14 percent level where it stood at the end of World War I, the 14,000,000 tons used in 1946 would have had to be increased to about 19,000,000 tons. Think of the added freight, bagging, handling, and other costs!

At the 1921 convention of the NFA, in an address on "Economic Factors in Higher Analysis Formulas," Loren W. Rowell, Swift & Company, Chicago, said:

> A 2–8–2 was considered a high-grade fertilizer not long ago, and the man who buys a 3–12–3 . . . can be sure . . . he has a guarantee that a high-grade plant food was used. . . . If a man buys two tons of 3–12–3, he will get axactly the same amount of plant food as if he had bought three tons of 2–8–2. [At prices then prevailing it was] the same as a reduction in price on 2–8–2 of $5.00 a ton.

At the conclusion of Rowell's address, the NFA voted to recommend the use of higher-analysis fertilizers.

Later in the same program Professor W. D. Hurd, then director of soil-improvement work, discussing "Some Tendencies in Soil Fertility Thought and Research," said: "When the Soil Improvement Committee started its campaign three years ago toward the elimination of undesirable analyses, and asked for support for high analysis goods, some [agricultural college and experiment station workers] were lukewarm; others actually hostile."

The plant-food content of fertilizer materials now available ranges from 5 percent in some organic nitrogen carriers to as high as 70 percent in a concentrated material like ammonium phosphate, which contains two plant foods—N and P_2O_5.

When the superphosphate industry was young, 12 and 14 percent finished material was common. Today, the average content is about 20 percent P_2O_5. It is, of course, completely wrong to assume that the other 80 percent is "inert" and "worthless." It is largely calcium, and it is not too much to say that throughout fertilizer history in the southeastern United States, until a few years ago, farmers were almost wholly dependent on the lime in their superphosphate for quickly available calcium. In these days, calcium is recognized as a necessary plant food as well as a soil amendment. At present, ground limestone, especially dolomitic limestone containing magnesium, is used as a fertilizer filler in order to even-weight the ton for sales purposes.

Educational work by the NFA and the agricultural forces of the nation, and economic causes, have led to the almost complete elimination of low-grade, or at least low-plant-food-content, fertilizers. Gradually the industry won the cooperation of the college, station, and extension staffs for its efforts to increase plant-food content, and today the average has risen to 21 percent of N, P, and K in compound fertilizers. The savings achieved are easily apparent.

Not only did the NFA bring about the hearty cooperation of the industry, but it coordinated the efforts of all other affected agencies in these matters. Two national conferences were held under its sponsorship, the first at Louisville, Kentucky, in September 1927, and the second at West Baden, Indiana, in 1928. Both were under the chairmanship of the late Dr. Tait Butler, editor of *Progressive Farmer* (Raleigh,

North Carolina) and formerly one of the most prominent state control chemists. Other groups, including the farm press of the nation, cooperated.

In the somewhat helter-skelter, competitive development of the industry, a tremendous number of grades or analyses came to be manufactured. In 1926 the NFA made a survey of the number of grades being made. Reports received included about 709 analyses, totaling 3,250,000 tons. The first 5 constituted almost 50 percent of the total; the first 10, almost 60 percent; the first 15, 67 percent; the first 20, slightly under 72 percent; the first 25, a little more than 75 percent.[4]

When the recently formed Association of American Fertilizer Control Officials settled down to business in October 1946, it recommended that future state laws restrict grades to a relatively small number, probably not to exceed twenty, and that the minimum plant-food requirement should be not less than 18 percent for available phosphoric acid, and not less than 20 percent for complete mixed fertilizers, except those that contain at least 25 percent of their total nitrogen in water-insoluble form obtained from plant or animal materials. An account of the creation of this Association is contained in *Commercial Fertilizer* (Atlanta: November 1946, 16–17).

During World War II, the Federal war agencies, in cooperation with manufacturers and state authorities, limited grades to as few as three mixtures in the case of Mississippi, plus a list of fertilizer materials applicable to all states.[5]

Statistical work. Early in its existence, the NFA recognized the value of statistics. In 1883 *The Fertilizer Movement of 1882–83* was published in Baltimore, the first of a series of annual publications. These volumes provide statistics of shipments from each important port or interior shipping center, giving tonnages by months and years for each railway serving fertilizer centers. They also provide figures on imports into the United States of crude brimstone, nitrate of soda, acetate, sulphate, phosphate, guano, bones, bone dust, and all other fertilizers. An interesting survey of the countries of destination is given for exports and imports; a separate chapter is devoted to the statistics of imports to the United Kingdom, with incidental information concerning a number of other countries. For some twenty years, the NFA has continued the statistics on fertilizer exports and imports and published them in its regular series of *Service Letters*.

As one of its early activities the NFA began the publication of the first comprehensive *weekly* wholesale commodity price index available in this country. It antedates that of Professor Irving Fisher, of Yale, and of course the present weekly index of the Bureau of Labor Statistics. For more than twenty years this index has been published regularly each Monday and appears as a separate release to the press and news ticker services and in the *N. F. A. News*. It is published in a large and varying number of papers, sometimes as many as 100. The well-known New York journal, the *Commercial and Financial Chronicle,* has published it every week since its beginning.[6]

The most complete record available on the monthly trend of fertilizer sales is that contained in the NFA report on fertilizer tag sales in 17 states. As these states use about 70 percent of the fertilizer sold, the report indicates the trend of consumption in the industry as a whole.

For many years the NFA was practically the only source of relatively complete national statistics of fertilizer consumption, which is the substantial equivalent of production, as there is very little carry-over of stocks. In *Recent Developments in the Fertilizer Industry,* published in 1930, the statistics were perfected by the use of all available data from 1910 to 1929. These were revised again in 1937 in *Fertilizer Consumption in the United States,* by Herbert Willett, then NFA economist. In recent years the data have been collected and published in cooperation with the U. S. Department of Agriculture.

Since 1925 NFA has issued a monthly report on production, shipments, and stocks

of superphosphate, the most important single fertilizer material. Working in collaboration with the Bureau of the Census, these statistics are unusually complete, probably representing no less than 95 percent of total production. The combination of NFA statistics with those supplied by nonmembers is now published regularly by the Department of Commerce.

Trends in farm income and prices, and their effect upon fertilizer sales, have been given constant study also.

Government intrusion into fertilizer manufacture and distribution. Shortly after World War I, the War Department completed two nitrogen plants at Muscle Shoals, Alabama. One was a cyanamide plant that produced approximately 1,700 tons of cyanamide, sufficient to warrant delivery of the plant to the government, The second was a modified Haber-Bosch direct-synthesis plant. It was so imperfect that it never went into production.

Beginning with 1920, there was constant pressure to inject the government into fertilizer manufacture, using the obsolete Muscle Shoals plants. By 1928 the proponents of government operation made their most determined effort to overwhelm industry opposition. The executive officer of NFA prepared a series of eight full-page advertisements, which were run in Washington dailies and a few farm papers. In those days, such advertising to educate legislators was a new and little-used method of petition.

A second method was the appearance in Washington of large numbers of fertilizer executives who came for frank and open conferences with their Congressmen. This procedure was referred to in the press of the day as "open opposition, openly expressed." As many as 100 executives came at one time, and NFA boldly issued statements listing their names, addresses, and business connections. The industry's efforts were well received by both the public and the press. Again, in May 1930, full-page advertising was resorted to.

At first, proponents of government operation were determined upon nitrogen production. While the debate was going on, private industry expanded nitrogen production so rapidly that everyone realized that government production was unnecessary. Then emphasis was shifted to government production of large quantities of phosphoric acid, although for sixty years the United States had been the world's largest superphosphate producer. The industry made every proper protest, and it was not until 1935, when indirect government invasion through creation of the Tennessee Valley Authority was put into effect, that fertilizer manufacture was actually begun—about 2,000 tons of 43–45 percent P_2O_5. Nitrogen manufacture (ammonium nitrate) was not begun until World War II came with its tremendous requirements of nitrogen for explosives.

Research on chemical problems. Encouragement of research began in the earliest fertilizer industry organizations. Then the work related especially to problems of chemical analysis and chemical control, especially as to dependable procedures to determine the content of ammonia, phosphoric acid, and potash in materials and mixed fertilizers. With the formation of AOAC, this work was taken over largely by the state control agencies, agricultural colleges, and experiment stations.

About fifty years ago, manufacturers began to employ staff chemists. By 1908 their number was such that the annual convention of NFA voted to form a chemical section. About the same time the American Chemical Society invited industrial chemists to join it and asked NFA to assist in forming a Fertilizer Division of the American Chemical Society.

On July 7, 1908, NFA voted that each member-company should instruct its chemist to join the Chemical Society and cooperate with its Fertilizer Division. Cooperation still continues, and each year the Society devotes an important section of its program to fertilizer research, including control problems, methods for preventing reversion of P_2O_5, compatibility of various carriers of

N, P, and K in mixtures, and simplification of terminology.

The Chemical Control Committee, established many years ago, continues to handle relations with AOAC and fertilizer-control officials, and leads in handling chemical problems within the industry. The primary purpose is to protect the farmer and the manufacturer against fraud. The importance of chemical control is such that all states but one have statutes dealing with plant-food content. Every lot of fertilizer material used in manufacture must be tested, and every lot of mixed fertilizer sold must be guaranteed under severe penalties.

SOIL-IMPROVEMENT WORK

On October 12, 1905, at the twelth annual convention at Asheville, North Carolina, a special committee was appointed to devise means to obtain more funds to continue propaganda work. Statistics showed that the members produced about 3,000,000 tons of fertilizer annually, and the committee recommended a yearly contribution of 2 mills a ton, which would raise about $6,000. It recommended also that funds be sought from nonmembers and from miners and vendors of phosphate rock, pyrites, sulphur, nitrate of soda, and potash salts, and from manufacturers and processors of tankage and dried blood, cottonseed meal, fertilizer bags, etc.

At the seventeenth annual convention in 1910, discussions that had gone on for years came to a head. Following an address by Professor H. D. Haskins, Massachusetts Agricultural Experiment Station, Amherst, it was voted that propaganda work be started by fertilizer manufacturers in cooperation with the railroads, the experiment stations, and the state agricultural departments. In urging the policy that should be followed by the Propaganda Committee, Charles H. MacDowell, Armour Fertilizer Works, Chicago, stated that the work could be done better by the Association than by individual companies; that it should be conceived on broad, constructive lines; that it should be carried on by expert agronomists who could give unbiased information; and that the propaganda organization should intelligently, aggressively, and properly defend the industry against unfair attacks. Evidently, the perfectly good word "propaganda" soon lost caste, for shortly, when preliminary work began, John D. Toll, Philadelphia, was elected chief of the "Educational Bureau." Toll served the industry for many years, and after the death of William G. Sadler, Nashville, Tennessee, was secretary until 1925. From 1910 until his death in 1932 he had also served as editor of the *American Fertilizer*, a semimonthly trade magazine, and the *American Fertilizer Year Book.* C. A. Alling, Darling & Company, Chicago, served as soil-improvement chairman until 1915, when Warner D. Huntington, The Davison Chemical Company, Baltimore, was elected. He served for eighteen years.

The urge for educational work developed so rapidly in the Middle West that by the time of the eighteenth annual convention at Atlantic City in July 1911, President John T. Welch reported that he considered the formation of the Middle West Soil Improvement Committee of The National Fertilizer Association (Mr. Alling, chairman) the most important work ever undertaken by the industry. Mr. Toll was appointed secretary, and Professor Henry G. Bell, Chicago, agronomist.

In 1916 Bell was succeeded by Sidney B. Haskell, who came from Massachusetts State College, where he had been an instructor and professor of agriculture since 1904. He immediately selected as his assistant Ove F. Jensen, of Michigan State Agricultural College. In 1920 Haskell resigned to become director of the Massachusetts Agricultural Experiment Station, and Professor William D. Hurd, who had been his assistant, succeeded him. Hurd, also a graduate of Michigan State, had had a wide experience as an administrative and research worker in agriculture. He continued as director until his death in 1924, when he was succeeded by Harold R. Smalley, a graduate of Purdue University, who had entered the service of the Soil Improvement Committee at Kansas City in 1920.

Observing the useful work of NFA in soil improvement, the Southern Fertilizer Association began similar work in 1916. Both Associations conducted almost identical activities until they united in 1925. Thereafter until 1929 the Northern and Southern activities were continued in separate divisions. In 1929, when J. C. Pridmore, a graduate of Clemson College, director of the Southern work, resigned, Smalley was made chief agronomist and director of all soil-improvement work. He did distinguished work throughout his long period of service, including the great depression of the 1930s and almost to the conclusion of World War II. He died in February 1945; an account of his work may be found in the *Fertilizer Review* of January-February-March 1945.

Almost incredible changes have occurred in our agricultural institutions since the industry's educational programs began more than thirty-six years ago. Agricultural extension work was then in its infancy—there were almost no county agricultural agents, and no vocational agriculture teachers. It was impossible to foresee all the varied economic activities inaugurated in 1913 with the creation of the Bureau of Markets, U.S.D.A., and carried on by its successors, The Bureau of Agricultural Economics, the Agricultural Adjustment Administration, and the present Production and Marketing Administration. While there are only about 3,100 agricultural counties in the United States, the total extension staff as of September 30, 1946, was 10,868, including 4,409 white and 362 colored county agents and assistant county agents. There are also probably no less than 7,500 vocational agriculture teachers. The majority are agricultural-college graduates, engaged in teaching and advising farmers and their children concerning better agricultural practices, including the use of fertilizers. In one state alone a few years ago, county agents tested farm soils at the rate of more than 100,000 samples a year, and made fertilizer recommendations based on those tests.

Cotton was long the chief consumer of fertilizer. With government programs for reducing cotton acreage, it became necessary to find another market for the fertilizers formerly applied to cotton. In 1926 the country planted 45,839,000 acres to cotton; in 1946, less than 18,000,000. In the early years of acreage reduction, 700,000 tons of fertilizer had to find new outlets. The industry's cooperation with the agricultural forces switched consumption to pastures, peanuts, fruits and vegetables, and other food, feed, and industrial crops.

Soil-improvement work has covered many subjects and problems. Its constant aim has been to give the farmer the best information available to assist him in conserving soils and improving crops. It is possible to indicate its development only with some examples.

Research fellowships on agronomic problems. Research inaugurated or participated in by NFA attacked the whole problem of more efficient use of fertilizer materials and mixtures. Mostly, it took two forms: financing fellowships in certain universities and agricultural colleges and giving of grants-in-aid to institutions for experimental work.

In 1919 NFA began to finance fellowships devoted to specific problems. The first was established in Wisconsin under Professor Emil Truog, to study the effects of readily available fertilizers applied in different ways; e.g., broadcast, in hills, rows, under the seed, over the seed, at one or both sides of the seed, etc. The second was established in Vermont in 1920 to study the effect of different fertilizer ratios on the growth and maturity of crops, and it continued for three years. The third was established at Iowa State College to study the effects of distribution and concentration of plant food, and machinery for applying fertilizers to various crops. Later, a graduate research fellowship was established at Purdue University to study the effectiveness of fertilizers in controlling the root and ear rots of corn; another at the University of Maryland to study the economic use of fertilizers on the soils of the Norfolk series; and a number in Southern states.[7]

As an illustration of the second method, for several years around 1928–30 NFA

appropriated \$3,000–\$5,000 yearly to the agencies concerned in the U. S. Department of Agriculture and the state agricultural colleges to start work promptly on determining the proper methods of applying fertilizers, especially to row crops. Ultimately, the Department financed the work, and at one time \$80,000–\$100,000 a year was spent, with tremendous economic results.

Research committee on fertilizer application. As a result of data obtained between 1919 and 1925, and at the suggestion of the late Dr. J. G. Lipman, director, New Jersey Agricultural Experiment Station, NFA created the National Joint Committee on Fertilizer Application. This Committee brought together the expert ability of NFA, the American Society of Agronomy, the American Society of Agricultural Engineers, the National Association of Farm Equipment Manufacturers (now the Farm Equipment Institute), and the American Society of Horticultural Science. Each of these agencies had competent representatives on the Committee. Its work continues to the present day, and new types of fertilizer machinery and remodeled old types are giving farmers the benefit of the research. There are still on American farms probably between 5,000,000 and 6,000,000 fertilizer distributors of one kind or another, the vast majority of which do not place the plant food at the place in relation to the seed that will give the farmer the highest economic return. This illustrates the magnitude of the mechanical problem.

Pasture fertilization. When NFA inaugurated its pasture work in 1926, almost no chemical fertilizers were used on American pastures. The Consumer Survey based on 1938 indicated that by then over 1,400,000 acres, or 3.8 percent, of the plowable pasture land in 23 states received some chemical fertilizer. Reports from 28 agricultural colleges in 1938 disclosed that about 80,000 farmers fertilized permanent pasture. In addition, about 1,000,000 acres of supplementary pasture were fertilized. Soil-conservation workers reported that thousands of acres of submarginal land were being fertilized for new pasture seedings. Many farmers, especially dairymen, found, according to the Survey, that each dollar spent for fertilizer returned \$3.50–\$5.00 in increased pasture yield. The work was supervised by Professor John B. Abbott, NFA agronomist for New England, with headquarters at Bellows Falls, Vermont. Subsequently, it was continued by various experiment stations. It showed that crude protein in barn-fed silage cost approximately \$17.50 a hundred pounds, whereas crude protein derived from well-fertilized pastures cost approximately \$3.50 a hundred pounds.

Educational publications. These were prepared for distribution directly to the agricultural forces and, through fertilizer manufacturers and dealers, to such of the nation's 2,500,000 fertilizer-using farmers as could be reached. Some of these publications are *Mineral Hunger in Livestock,* hundreds of thousands of copies of which were distributed; *Putting Plant Food to Work; Fertilizer Industry Forges Ahead; Growing Quality Tomatoes; Producing Low-Cost Cotton; Making Pastures Pay in the Northeast; Green Acres; Organic Matter: The Life of the Soil.* Since 1911, no less than 300 pamphlets have been issued. A careful estimate made several years ago credited the present NFA with distribution of more than 10,000,000 copies.

Hunger Signs in Crops. This is the title of a book published under the sponsorship of the American Society of Agronomy and NFA, and completely financed and supervised by NFA. It contains 9 chapters written by 14 distinguished scientists, who received no compensation; about 350 pages; some 80 full-page color illustrations; and 95 half tones. The NFA Board of Directors authorized the printing of 10,000 copies. Prepublication orders for 6,500 copies convinced the executive secretary that a larger edition could be sold readily, and he personally guaranteed the cost of an additional 4,000. Beyond doubt it is the world's outstanding text on the symptoms of malnutrition in crop plants. Nearly 22,000 copies have been sold, requiring a third printing.

Plant-food research problems. In June 1936, the Board of Directors authorized amending the bylaws to provide for a Plant Food Research Committee to consist of 15 industry agronomists. Among the committee's duties were coordination of research work already under way, and recommendation and conduct of new projects. One of its early activities was to assist in planning *Hunger Signs in Crops.* In the eleven years of its existence, the Committee has paid special attention to diagnosis of symptoms of malnutrition in crops, peanut fertilization, heavier-than-normal applications of fertilizer in crop rotations in the Middle West, fertilizing corn in the South, fertilizing fruit trees, plow-sole fertilization, study of granular fertilizers, soil testing, and quick methods of testing plant tissues to determine plant-food deficiencies.

Moving pictures. This activity was inaugurated more than twenty years ago. In a recent year 108 copies of three moving pictures in Kodachrome were in circulation. By 1945 they had been shown to more than 1,000,000 agricultural people in 46 states and several foreign countries.

Nitrogen utilization studies. In September 1942, Dr. William H. Martin, dean, College of Agriculture, and director, Experiment Station of New Jersey, suggested that when the war was over the United States would have a large surplus of nitrogen-fixing facilities, and that steps should be taken to ready agriculture and the industry for their effective use. Accordingly, the executive officer of NFA took steps to institute the necessary organization. The first meeting of the National Joint Committee on Nitrogen Utilization was held in Washington in the fall of 1942; the second, in Cincinnati in November 1943. More than 225 agriculture and industry representatives, including research agronomists and horticulturists from nearly all states, attended the second meeting. The Committee is composed of not more than 5 persons from each of the following organizations: Association of Land Grant Colleges and Universities, American Society of Agronomy, American Society of Horticultural Science, the Tennessee Valley Authority, U. S. Department of Agriculture, Society of American Foresters, and The National Fertilizer Association.

Some of many other soil-improvement activities can only be named: County-agent soil-improvement contests; conferences with agricultural workers; agronomic charts; filmstrip lectures; and preparation, circulation, exchange, and sale of natural-color slides showing nutritional deficiencies.

SURVEYS AMONG FERTILIZER CONSUMERS

In 1928 NFA, assisted by 56 companies and nearly 1,000 of their salesmen, interviewed more than 48,000 farmers on their experiences in 1927. *American Fertilizer Practices,* a report of 157 quarto pages, analyzed the returns. It disclosed how many and what papers are read by farmers; what bulletins on the use of fertilizer they received from state experiment stations; what booklets, folders, and advertising matter they received from fertilizer companies; the extent to which farmers themselves have conducted experiments with fertilizers; how many visited experiment stations, agricultural meetings, and demonstration farms; agencies helpful in selecting fertilizers; effect of fertilizers on crop quality; amounts of fertilizer used per acre; acreage of specific crops fertilized and quantities applied; and farmers' estimates of increased yields obtained by the use of fertilizer.

A second survey was made in 1939. *American Fertilizer Practices, Second Survey,* gave the results. More than 32,000 farmers were interviewed by 656 salesmen and field men. At this time farmers were spending nearly $210,000,000 annually for almost 7,500,000 tons of fertilizer. In 1946, according to advance estimates, they spent approximately $440,000,000 for more than 14,000,000 tons.

These surveys provided information that was indispensable in planning agricultural programs involving fertilizers during World War II.

At the request of officials in the U.S.D.A., a third survey was undertaken in 1944 to

cover the period of World War II. Because of lack of manpower and gasoline, only about 15,500 farmers were interviewed. Additional data were obtained from 555 vocational agriculture schools in 42 states covering 13,600 farms. The results have been published in part in the *Fertilizer Review* and in mimeographed form.

Public relations. In the early years questions of national policy were handled by the NFA staff under the Executive Committee and later the Legislative Committee. When necessary special subcommittees were appointed, such as the Potash Committee, created in 1909 to deal with contract problems arising from the growing regulation of the potash industry by the German government.

After World War I (1920–26) the Washington Committee (operated in cooperation with the Manufacturing Chemists Association) handled such duties. Then many new problems loomed on the industry's horizon, including Muscle Shoals, a proposed Federal fertilizer law, the appearance of the first definite farm bloc, and a bill to appropriate $10,000,000 to finance government purchase and sale to farmers of Chilean nitrate of soda. This last proposal arose from the success that had attended a similar activity during World War I.[8] Since 1927 all such work has been carried on by the Public Relations Committee.

War work. During World War I, NFA was not staffed to provide the full services now expected of a trade association. The same was true of other chemical-industry organizations. In May 1917, the Council of National Defense set up a Committee on Chemicals. When the War Industries Board succeeded the Council, the Committee became the Chemical Division, of which Charles H. MacDowell was chairman. In the meantime, the chemical industries organized The Chemical Alliance, Inc., with 12 sections, of which the Fertilizer Section was the largest. The Alliance cooperated with the War Industries Board throughout the war. Horace Bowker, later president, The American Agricultural Chemical Company, New York, the largest fertilizer manufacturer, was the second president of the Alliance.[9, 10]

During World War II, the members and staff of NFA were in constant cooperation with the official agencies responsible for handling agricultural problems. The executive officer assisted in the selection of the Fertilizer Industry War Emergency Advisory Committee, and subsequently in the selection of contact committees for the War Production Administration, the War Food Administration, and the Office of Price Administration. As a matter of public recognition and historical interest, the names and connections of the industry executives who served the government through government-industry committees during World War II were listed in the *Proceedings of the Twentieth Annual Convention of NFA* (1944, 101–2).

Throughout the war and after, at the request of the government war agencies, NFA issued *Fertilizer War Notes.* These informed the industry and agricultural officials concerning supplies, distribution, government actions, policies, and regulations. They were sent regularly to more than 2,500 addresses. NFA also provided government agencies with statistical and other information as to productive capacity, processes, grades, plant-food content, rates of application, sales procedures, trade practices, materials, containers, and commercial and technological operations of the industry.

Dues and expenditures. Association funds have been raised chiefly through dues on tonnage of mixed fertilizers and superphosphate sold; through voluntary contributions from producers of potash, nitrogen, and sulphur; and from fixed annual dues of associate members. The rates of dues varied from 4 cents a ton (1925–26) to 0.75 cent in the depression (1931–32). They rose quickly during the period of the N.R.A code to 5 cents a ton, and later receded to 2.5 cents for the period ending in 1945.

In its first twenty years, the present NFA expended approximately $3,750,000 for all types of cooperative industry work. Soil-improvement work received the largest appropriations, totaling more than $1,250,000.

In 1925–26 total expenditures were $183,-824. Owing to heavy emphasis on soil-improvement work, they rose to $243,839 in 1928–29. They fell to a twenty-year low of $58,312 in 1932–33, and then rose under the N.R.A. code to $276,735 (1934–35). During recent years they have ranged between $150,000 and $200,000. The average of expenditures for the period of twenty years was somewhat less than $185,000.

Variations in membership. Like some other associations, only a few of which still exist, NFA originated out of the economic depression of 1893. C. J. Judkins, Department of Commerce, has estimated that there were 120 national trade associations in 1894, 400 in 1910, 1,100 in 1930, and between 1,800 and 2,000 in 1946.

After the present NFA was formed in 1925, it had 168 active and 74 associate members. In the depth of the great depression in 1930–31, active members were 132 and associate, 54. The maximum number occurred in the period of the N.R.A., when there were 525 active and 44 associate members. In 1944, at the peak of the war there were 399 active and 70 associate members.

SPACE limitations prevent more than a mention of many additional activities such as insurance work, begun in 1909 and discontinued in 1933; cost accounting activities, begun in 1907 and discontinued in 1941; traffic and transportation work conducted throughout the life of NFA but carried on energetically since 1925; work on tariff problems, particularly prior to adoption of the Fordney-McCumber Bill in 1922, the Smoot-Hawley Bill in 1931, and when trade agreements were being negotiated actively in 1934–36; sales conferences and dealer education; surveys of sales opportunities; and defensive litigation such as the suit commonly known as the "South Carolina Compulsory Open Formula Case," which NFA carried through the Supreme Court of the United States; cooperation in trade practice work under Federal Trade Commission conference rules; preparation and administration of the National Recovery Administration Code of Fair Competition (1933–35); Department of Justice investigations (1939–41); and preparation of a bibliography on the influence of fertilizers on crop quality.

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ONE ATOM AND MANY*

J. H. MANLEY

Dr. Manley (Ph.D., Michigan, 1934) is on leave from Washington University to devote his time to the problems of atomic energy. Secretary of the General Advisory Committee to the Atomic Energy Commission, Dr. Manley divides his time between Committee duties in Washington and the Los Alamos, New Mexico, Scientific Laboratory, where he was formerly head of the Division of Research.

THE physical scientist is accustomed to speak of interactions. The physicist or the chemist will think of a hydrogen atom and of its interaction with another to form a hydrogen molecule. He will think of the interaction between hydrogen, carbon, and oxygen atoms as stabilizing a complex organic molecule. He may occasionally think of the interactions between many such molecules which, so far as elements go, make his own body. He will probably be cognizant of other interactions that make him a human being, but I am afraid that he oftentimes neglects those interactions, especially in the broadest sense.

The word "interaction" is a fine word. It will give me an excuse to say many things which are tied together by many kinds of interactions. There has been much written about the interaction of science on human society; there has been much written about atomic energy and the atomic bomb. Perhaps I have been unduly impressed with this development because I have had a part in it, but, nevertheless, it is my thesis that in this endeavor we have a most striking illustration of the interaction between the achievements of science and the individuals and institutions of society.

I should not like simply to repeat what has already been said, although that is, to a certain extent, unavoidable. At least I may take a different approach. Let me take one atom, the one called U-235 and first of all consider the relation between this one atom and the many atoms that make a nuclear physicist—myself. Later, lest I be thought selfish, I shall use the many-atom concept to denote all of us.

* From the John Wesley Powell Memorial Lecture, given at Colorado Springs on May 1, 1947.

My story opens on June 10, 1940. On that day this particular assembly of atoms held together by reasonably well-understood interactions, this nuclear physicist of whom we speak, was in his laboratory investigating a relatively new interaction, the interaction of neutrons with atomic nuclei. Suddenly, from the radio: "FLASH: Mussolini attacks France." Sound waves to the ear, response of the aural nerve, messages in the central nervous system, muscular reaction—a kick and a dent in a can of borax powder—the physical evidence of an interaction between an event 5,000 miles away, this particular nuclear-physics laboratory, and a scientist in it. One could hardly attach profound significance to this little incident at the time, but now it seems somewhat symbolic. World events had invaded the laboratory. Work in that laboratory for the pure, unadulterated benefit of science did not long continue. Everywhere scientists began to leave the laboratories and the academic halls. A strong interaction between society and science, between situations and the scientist, was making itself felt. With other things of science, the interaction of a neutron with a nucleus was no longer just a matter of scientific curiosity, it was part of a matter of life or death for democracy—an institution of society based on the dignity of the individual. In time the apparatus of that laboratory, including the dented can, found its way to a New Mexico mesa, a war-born Shangri-La, Los Alamos. So did the physicist, but by a more devious route. He found himself at the newly established Metallurgical Laboratory of the University of Chicago, to use his acquaintance with neutrons for something more than just the publication of an article in a relatively little-read scientific journal.

Although nuclear physics and the workings of a nuclear physicist are familiar to many, it may be neither uninteresting nor irrelevant to our subject to digress in order to indicate the role of the nuclear physicist in attaining that outstanding scientific achievement—the first sustained nuclear chain reaction, the continuing, controlled release of nuclear energy. In so doing, however, I must emphasize that his role was that of a member of a team comprised of other physicists, of chemists, mathematicians, biologists, metallurgists, engineers, geologists—representatives of nearly every branch of science.

What is this chain reaction, what makes it go, what do neutrons have to do with it? It would hardly be appropriate to expand to a graduate lecture on nuclear physics, but perhaps with a little fancy we may approach an understanding of the nuclear physicist's role.

IMAGINE a village populated with U-235 atoms, female elements, if you like, homebodies that have an interest in a particular physical process—fission. In this village are some neutrons, male elements, rather averse to being tied down and rather interested in a bit of philandering. All atoms have strings attached, electrical interactions that make them interested in electrons and in other atoms, but the neutron is an exception. He goes to the heart of matter, the nuclei of atoms, before he shows any interest in the other residents of his world. So our poor, lonely U-235 atom, like all others, must wait patiently for her chance to capture a wandering neutron. But, unlike others, when she succeeds, the marriage is consummated by a rather unique event: Lady U-235 disappears with a flash of energy into two radioactive wenches and a few neutrons. The word "few" is quite important here, for if "few" did not mean "more than one" the chain reaction would not be possible and the world would have to get on with more conventional kinds of energy.

Assuming that our village possesses enough U-235 residents so that the number of deaths does not appreciably affect the number of marriages for a long period of time, then the problem of obtaining a chain reaction is the problem of making the neutron population grow to a desired value and then become constant. Unfortunately for the ease of attainment of this goal and for the satisfaction of Miss U-235, a wandering neutron is subject to a number of possible diversions that may prevent the establishment of a brief, but presumably happy, home life with a U-235 atom, and the birth of a new generation of neutrons. He may wander off into the woods around the village and die a hermit, having had no truck with the beauteous U-235. He may die in the arms of seductive but sterile atoms that await him along the graphite streets of the village. One particularly unfair but interesting cause of his death occurs in the same household with Miss U-235. She has an older sister, U-238, normal life a few billion years, not without charm of her own, but she begets no neutron children. Her union with Mr. Neutron produces the new element plutonium.

Nature has provided the elements for a stable population—the neutron and the fertile U-235 atom—but she left to man the task of solving the problem of village design. It was necessary for the scientist to collect suitable constructional materials, lay out the streets, put in plumbing, assign quarters to the Misses U-235, shelter the Misses U-238 to give the younger sister a chance, remove the dangerous sirens intent on enjoying the company of a neutron without the responsibilities of a younger generation, and make the village large enough so that only a few neutrons would find their way beyond its limits. And then, of course, since the scientist wished to control his planned society, he provided a troupe of itinerant chorus girls, the Cadmium Sisters, to go into the village at any time and calm any tendency for things to get out of hand between the neutrons and the Misses U-235 by taking out of circulation some of the neutrons of each generation.

As everyone now knows, after months of strenuous effort on suitable materials and different village plans, the goal was achieved on December 2, 1942. On that day the planning of the scientist bore fruit. Some of the Cadmium Sisters were called out of the village, more neutrons found U-235 mates, the

population increased, and then the village settled down to a stable population of determined size, each birth exactly replacing a death, and each generation contributing to the power output and plutonium production of the village. The interaction of the scientist in the life of one atom had given a new form of energy to the many human atoms that populate this sphere.

The public did not hear of this event, the press teletypes were not full of the momentous news. Mussolini had not just attacked distant France, but Tojo had attacked Pearl Harbor. An interaction between elements of human society had cloaked in military secrecy an achievement as important to mankind as the discovery of fire.

Four months later the locale changed. Our nuclear physicist, with a truckload of that laboratory apparatus of which I spoke, was riding up a narrow, crooked, bumpy road toward the top of a New Mexico mesa. The problem ahead was the construction of a new planned village, a village in which the neutrons do not stroll, they run; a village of no streets and spacious lots, but of closely packed U-235 atoms. The object was to arrange that the whole village be consumed in one sudden burst of energy in an amount more than that stored in thousands of tons of TNT.

To do this there was assembled the best-equipped nuclear-physics laboratory in the world—Van de Graaff generators, high-voltage machines, cyclotron, betatron, chain-reacting units. And this was only the physics section. There were facilities for chemistry, metallurgy, and various strange new processes. It is my guess that that laboratory turned out the equivalent of a Ph.D. thesis every week or so. In the mind of each scientist was always the question, Is there time? There were twenty-four-hour shifts, frayed nerves, depths of gloom, and heights of achievement.

I find it difficult to limit myself in telling this story, so much was packed into those few years. There were not only the things of interest to the scientist, there were things of interest to any curious individual. We might explore, for example, the social interactions within this new isolated community of Los Alamos—a community in which it was the policy to encourage wives to work, not just to keep the number of inhabitants as small as possible, but as a preventive treatment against the dangers of an enforced isolation of which they did not understand the purpose. Travel, except for business, was permitted only within a well-defined region. The world for us was bounded as it was for the ancient Pueblo Indians—by the North, South, East, and West Mountains. City attractions were supplied by Santa Fe, but, even there, if a lone scientist wished to relax in the La Fonda bar, he had first to peer in and count the number of occupants from Los Alamos. If there were five he might join them in comradeship; if there were six he had to drink in solitude with no glance of recognition toward his fellow-citizens.

But I must carry my story on to the spring of 1945. The technical work had changed by this time. Scientists were designing and building special recording equipment. I was no longer studying the behavior of neutrons; a giant popgun for making shock waves had taken the place of a neutron source in my laboratory. As the spring progressed there was a marked decrease in the male population at Los Alamos. One wife would say to another, "I haven't seen Bob lately," and the other would reply, "He's away." No more than that, for that was all she knew. Then a few days later Bob would appear, kiss his wife, and make a beeline for the shower to rid himself of sweat, grime, and gypsum. Bob had been at "Trinity."

Trinity was the code name for a corner of the Alamagordo Bombing Range; there in the desert, shelters were being constructed, field stations laid out, miles of wire strung. It was quite a place, a few hundred square miles of sand and bush under close guard and alive with scientists and their helpers. I was told that, if a lonely sentry issued a challenge to some nocturnal disturbance and the reply was "Moo," no further challenges were given, but a shot would ring out in the stillness. My only scientific observation in this regard was that the camp seemed well supplied with beef.

In early July it appeared that if we could keep up the effort we might make a favored date. It was not simply a case of firing when everything was ready; weather was an important factor. Our meteorological section favored July 16, and the weatherman was respected since none of us was disposed to have a deadly radioactive cloud settle down around his head. It was a grand rush, but the nights before the fifteenth found us making dry runs—complete tests of every operation except a firing signal all the way through to the bomb. The betting pool on our success or failure was closed. Many a scientist had gambled a dollar on his ability to predict the number of tons of TNT equivalent. There were some of little faith, for the number zero was not without a few dollars.

The evening of July 15 brought a fine collection of desert thunderstorms into our valley from every direction. Had our medicine man given up the ghost? Lightning struck around the tower on which our gadget rested uneasily, and this did not contribute to the peace of mind of those working underneath. Would we fire? The weather certainly looked anything but good, but it would be days before conditions would be favorable again. For both photographic and meteorological reasons the shot was to take place at night, and 3:00 A.M. had been chosen. In the early hours of July 16 it was finally decided to postpone the firing time to 5:30 A.M.

I was an occupant of the shelter at West 10,000, six miles from point zero and the tower holding the product of not only Los Alamos but Berkeley, Chicago, Hanford, and Oak Ridge. The doors of the shelter, nailed open as a precaution against the blast, looked away from zero. The inside was lined with apparatus to record nearly every effect imaginable—blast, light, heat, earth shock, and radioactivity. Near the door was the screen of a projection periscope on which we would see what happened at 5:30.

By 3 o'clock the strain of the past days and weeks and months was beginning to tell. I was the only one awake in this shelter and I would have gladly given up the responsibility for the shelter in order to catch a bit of sleep. Everything had been checked; this was no time to speculate on the interaction between this event and the world, and sleep was a very sensible thing.

At 4:50 I woke the forms huddled on the floor; the guards came in from the area and the roads; each individual assigned to this shelter was accounted for; and similar reports from the other two shelters brought the announcement that the area was certified clear, the arming had been complete, everyone was under shelter except those ten miles or more away. The loud-speaker continued announcing time—minus 20 minutes, minus 15, and so on. At minus 45 seconds the robot mechanism removed all human responsibility except for that which would result at time zero. I remember vividly minus 2 seconds, then time seemed to stop.

I have been told of John Wesley Powell, his trip, and his difficulties in describing the country around the confluence of the Colorado and Green rivers. I know how he must have felt. There are sights that do not transfer easily to the written or spoken word. I have never seen the light of 1,000 magnesium flares, but perhaps what I saw that morning was comparable. For a brief instant the region around point zero was hotter than the sun. Our periscope screen was blinding. The whole valley, seen through the open doors of the shelter, was as bright as at noonday. I think I said, "Gentlemen, you have seen something. Stay inside for the blast." For a full half minute we waited while it sped across six miles of sage and yucca. Then suddenly there was a wind in our shelter and a roar as the sound filled the whole valley and echoed back from the mountains. Everything was over except for dashing outside and watching the colored cloud mushrooming into the dawn sky.

When the blast came, Enrico Fermi, outside at ten miles, was dropping bits of paper from his hand. I suppose his companions thought the usually imperturbable Fermi was nervous, but from the angle of fall of the paper as the blast swept by he quickly announced an approximate figure for the energy release of the bomb. The lack of that genius forced me to climb to the top of the shelter and examine one of the microbarographs

there. The result was clear, the optimists in the betting pool had won.

The El Paso paper carried a few stories of a strange event: a locomotive engineer on the Santa Fe had seen a great light in the sky; a woman, driving eastward from Arizona into New Mexico thought that the sun rose, set, and rose again that morning. The next day there was an official announcement: "An ammunition dump in a remote section of the Alamagordo Base blew up with pyrotechnics. No one was injured." This was the public announcement of the most astounding experiment in history—the sudden release of a vast quantity of energy stored in quite a small package, the energy of matter itself. The scientist had brought to earth for a minute fraction of a second the temperatures and pressures of the stars.

In the high councils of government the news was startling—the two-billion-dollar gamble, authorized by one who had faith in knowledge, had paid off. The way was now open to bring the war to a swift but awful conclusion. The first application of atomic energy in the affairs of men must involve the death of some that others might live. One atom was no longer a scientific curiosity —it was to show its interaction with man in a most drastic, vivid, and fearful way.

Much of what I have related is already well known. Surely everyone is familiar with the events from that July day until now. Let us therefore glance at the future. A scientist does not like to do this—he prefers to leave it to the feature writer—but I have presented a thesis: that the development of atomic energy is a most striking example of the interaction between the achievements of science and the institutions of society. Our story has suggested some things; we must guess at the future for others.

I have the feeling that most people have been sufficiently exposed to forecasts of the effect of the military application of atomic energy and that they would be pleased if predictions concerning atomic war were omitted. This I shall do.

I also have the feeling that I may be expected to paint a rosy picture of the future with atomic energy as a source of energy, of a future with autos and home-heating plants run by uranium pellets, of an age of plenty based on atomic power. This I shall not do. In fact, I cannot. If I am pressed, I shall say that much of the speculation on this aspect is simply impossible because it is incompatible with the facts of nature. Atomic power is possible, yes. A plant that is not small can be built. Its waste heat might heat your homes. Its power might charge a battery to run your electric car. It will not be a strikingly cheap source of power, and it will not be available tomorrow or in one year. My guess is that in ten years we should see a successful unit producing some kind of conventional power—electricity or steam—on a modest commercial scale. I do not believe that these statements are ultraconservative or unrealistic. If I am being unwittingly biased, it is because I am frequently distressed by excess enthusiasm in things I read, things usually written by those who have no intimate connection with the development work in this field.

Does this mean that the whole phenomenon of atomic energy is much overrated? I think not. As a source of energy it will be important—how important is difficult to say. But energy is an obvious feature of this new discovery. I should like to consider some others that are less so, but which, to my mind, hold great promise of benefit to mankind. Since we are accustomed to think in terms of physical benefits—health, leisure, and technological sedatives for the difficulties of living—let us choose a few examples, examples obviously chosen by a scientist.

One of the aspects that interests me is our sudden wealth in available neutrons as the result of the operation of chain-reacting units. Of many things that can be done with neutrons, I shall choose two and merely suggest where these two things may be the beginning of new paths in our world of knowledge.

A hydrogen atom in a sodium hydride crystal has revealed its presence by diffracting neutrons. X-rays, which we usually think of as waves, have long been a useful tool in revealing the locations of atoms in molecules. But X-rays, electromagnetic in nature, are

affected by the electrons of an atom, and the one electron of the hydrogen atom produces only a small effect. The neutron, on the other hand, pays no attention to the electrons of an atom, but only to the nature of its nucleus. For a neutron, therefore, the nucleus of a hydrogen atom may be quite as important as that of some other element. If such a neutron-effect helps us learn about the location of hydrogen atoms in complex molecules, who knows what may derive from this new knowledge?

There is another curious but true fact about neutrons. While bounding around inside a material at room temperature, they may have temperatures even up to and beyond thousands of degrees centigrade. Temperature has been a tool of the chemist and the chemical engineer for a long time. Can neutrons provide a cold heat which may be useful in catalyzing new, interesting chemical reactions? It would hardly be sound to think of our crude oil of the future being cracked to gasoline in a bath of neutrons, but many chemicals are used in far less bulk than gasoline. Who knows what will happen if an atom is knocked out of a hormone by a hot neutron?

Another aspect, one that is always mentioned, is the greatly increased abundance of radioisotopes. Since many uses have already been publicized, I shall take but one example.

Through the use of radioactive carbon it has been established that a certain enzyme can utilize carbon dioxide directly in the production of more complicated molecules. I am not a biochemist or a biologist—I've even forgotten which enzyme and which molecule—but I am intrigued by the thought that other living organisms than green plants may perform similar functions. This is but one example of a possible path along which this new tool may lead us to a greater understanding of the processes in living things, perhaps even of life itself. Small wonder that an eminent biologist has characterized the radioisotope as the most important tool in his field since the discovery of the microscope.

When these examples are multiplied, as they can be, I am led to think that the new things that have been brought to science and to scientific research by the release of atomic energy may prove of greater benefit to all of us than just the energy itself. At least we must not lose sight of these possibilities.

I have said that we are accustomed to think in terms of the physical benefits of atomic energy. I am afraid that this is largely true, but I am not happy that it is so. Our story began with an interaction between a political event and a tin can in a laboratory where a purely physical interaction between neutrons and nuclei was being studied. It continued with the interaction of a neutron-U-235 system on a nuclear physicist within a framework supplied by the political and social interactions of World War II. It has attempted to suggest possible future interactions on the welfare of men arising from the development of atomic energy. Is this where we stop? Are we, you and I, uninterested in anything more from a great scientific event? Can it be that we are so enchanted by technical achievement that we see only that? Are we not individuals concerned with interactions between ourselves and our fellow-men?

The road of history along which we travel is bounded by shadows. Occasionally a great achievement or a great person throws a spot of light into the shadow, which illuminates not just the future things for man but his future nature. The light is not seen by many, only those whose eyes are rapidly adaptable. An awful light in a New Mexico desert has made some close their eyes in fear. Its brilliant illumination has made others see more clearly than ever before the import of a faith which is of the nature of man—the faith that he, within himself, has the power over his own destiny. What clearer example could he need than his ability to release the very force that holds the elementary particles of matter together, that makes this earth through the energy supplied by our sun a living place for human beings?

Recent events will, I think, give us a picture of where, as men, we are. It would be unfair if I were to gloss over those that have been and remain discouraging: The wrangling over domestic legislation for atomic energy; the failure to reach international agreement on control; the sorry spectacle of

Congressional debate on the confirmation of the Atomic Energy Commission; the fact that we are still committed to the insecure path of security through superiority in atomic armaments. But there are things that are, after all, encouraging: The mere fact of the writing of the Acheson-Lilienthal Report, with its meeting of minds of scientist, businessman, industrialist, and public servant; the position before the world in the Baruch proposal that this nation is willing to restrict its national sovereignty for the benefit of all; the passage of national legislation with the wording,

> it is hereby declared to be the policy of the people of the United States that . . . the development and utilization of atomic energy shall . . . be directed toward improving the public welfare, increasing the standard of living, strengthening free competition in private enterprise, and promoting world peace.

I want to believe that these words of the Atomic Energy Act of 1946 are not to be interpreted within the narrow framework of technological benefits, but that they are a statement of recognition by our people that science and technology must have a moral purpose—they must continually increase the importance of the individual.

I suspect that John Wesley Powell in his search for land for his fellow-men must have seen some of the vision—the dignity of man as reflected in his self-sufficiency, his control over his own destiny on his own land in harmony with the nature around him. Science and technology expand the need for harmony—what Powell could suggest and one man could do for himself seventy years ago, a T.V.A. is doing for many men today. I am inclined to the thought that it is not a mere act of chance that the man responsible for carrying out the vision of T.V.A. is now Chairman of the Atomic Energy Commission. It is my hope that atomic energy, involving the ultimate force of our physical world will stimulate and combine with the visions of men to mean far more in the way of human dignity throughout the world than any of us can now foresee. It will be a part of our life and our obligation to watch and understand the interaction of one atom on many.

THE JOHN WESLEY POWELL LECTURE

The John Wesley Powell Lecture was established in 1929 by the Southwestern Division, American Association for the Advancement of Science, in honor of the first man to explore the Grand Canyon of the Colorado River. The Powell Lecture is delivered on the opening night of each annual meeting of the Division by a distinguished investigator in some field of science, on a topic of his selection.

Major Powell's interest in the natural sciences was manifest early in life. When his family moved from New York to Illinois, Powell joined the Natural History Society, making collections and serving as secretary.

After the War Between the States, in which he lost his right arm at Shiloh, he was appointed professor of geology at the Illinois Wesleyan College. There, concerned with the future welfare of other comrades handicapped by war, he remotely envisioned the basic principles of land settlement and reclamation now practiced in the Western states. Pursuant thereto, in 1867 and '68, he led geological expeditions into Colorado and Utah, making comprehensive studies of the climate and the land and water resources of the upper Colorado River basin. Inability further to follow the Green and Colorado rivers by land through their rugged canyon courses led to his plan of exploration by boat. In May 1869, under the direction of the Smithsonian Institution, Major Powell began his Geographical and Geological Survey of the Colorado River by boat. In the course of this expedition his party passed by boat through the entire length of Grand Canyon—a hazardous feat first described in his *Exploration of the Colorado River of the West, 1875.*

Major Powell was later engaged in geological and ethnological explorations in Arizona and Utah and from 1874 to '79 carried on an agitation for a reorganization of the many rival surveys in the West. This led to the establishment of the United States Geographical and Geological Survey in 1879. He was at once made director of the Bureau of Ethnology and two years later became director of the entire Survey. He contributed importantly to organization of the early work of the Survey, and his many valuable contributions to ethnology are found principally in its reports.

Major Powell resigned in 1894; in 1902 he died at the age of sixty-eight.—(*From the introduction to Dr. Manley's lecture by Dr. Jesse L. Nusbaum, senior archeologist, National Park Service, and consulting archeologist, Department of the Interior, Santa Fe, New Mexico.*)

THE PRINCIPLES OF POOR SPEAKING

HAROLD F. HARDING

Following World War II, in which he served as a colonel in the Pacific theater, Dr. Harding (Ph.D., Cornell, 1937) studied on a Rockefeller postwar fellowship in the humanities. He joined the Department of Speech at Ohio State in 1946. Dr. Harding was recently elected editor of the Quarterly Journal of Speech.

CONTRARY to popular opinion, it is fairly easy to write or to speak well. But writing poorly and speaking poorly are really difficult. To give these arts their proper due requires hard study.* Many accomplished practitioners of poor speaking are not aware of their methods. It is time that poor speaking be given the systematic treatment it deserves.

The serious student of poor speaking can well afford to consider these first principles:

1. Make no preparation in advance.
2. Give the speech no order; let it ramble.
3. Avoid a conclusion.
4. Mumble your words and don't look directly at the audience.
5. Never analyze an audience and never evaluate your performance.

PLANS AND STRATEGY

Preparation is irksome and time-consuming. Therefore, don't prepare until the night before you are to deliver the speech. Better still, give your speech impromptu and do your preparing on the spur of the moment. A sensible plan is to give again a ten-year-old lecture without revision.

Never make a study of the kind of people in your audience, and don't adapt your speech to their intelligence or their interests. When you begin, don't bother to define any new technical terms. Throw out three of four new words at the start and give them unusual or unheard-of pronunciations. This will distinguish you as an erudite speaker.

In planning an introduction, don't attempt to narrow down the subject of your talk. Cover the whole field, giving the early and the recent history. Don't omit a detail. Ability to dissertate on the entire background will mark you as a person thoroughly familiar with the tradition of your subject.

Read your speech, it's far more scholarly; don't try to master its ideas in outline form—that's the surest way to keep your audience awake. Whenever you read from manuscript, don't read it aloud beforehand. This will enable you to speak with your nose close to the manuscript. If you do weaken and speak extempore with the use of notes, don't go through the speech in advance; above all, pay no attention to any set time limit. If you use up your allotted time in the introduction, you can always invade the time of the rest of the program. This adroit maneuver will serve to delay the proceedings, and it will certainly cause people to remember you as the speaker who was full and flowing over.

Modesty is old-fashioned, so use "I" frequently. The word "my" at the beginning of successive sentences always attracts attention.

Personal appearance has little relation to what an audience will think of you. Be sloppy in your dress, or be flashy, as you prefer. Stand directly behind the speaker's stand. Have the light adjusted so that it restricts the audience's view of you while speaking; then begin to mumble, holding your head in an attitude of reverence.

GENERAL RULES OF COURTESY

Be late in arriving for the session and make enough commotion to attract notice when you enter. Be sure to shake hands with friends on the aisle while your predecessor is talking.

If a public-address system is available, avoid it. If one is not available, complain

* See the enlightening article, "The Principles of Poor Writing," by Paul W. Merrill, in THE SCIENTIFIC MONTHLY, January 1947.

that you cannot speak decently without one; then proceed in a slow monotone. If you speak before a microphone and to a radio audience, let the audience before you go hang. Your outside audience is greater and, naturally, far more important.

As for tempo, or speaking rate, try to cultivate extremes of either 75 or 200 words a minute. Avoid 125 words per minute—it's a dull rate, and, if you should enunciate distinctly, you gamble on having your hearers understand individual words.

If you have a specific purpose, conceal it. When you use charts or diagrams, make them small and the lettering faint. Talk to the chart rather than to the audience. If you use slides or film strips, make certain that the projector does not function. This will allow you to make small sketches on the blackboard with your back to the audience while you talk in a low, confidential voice.

Writers of textbooks on speaking always harp on "conversational quality." It's a flat failure in poor speaking. To succeed, either talk to yourself or make an oration. Conversing directly with the audience is just another one of those impractical modern theories.

A sure-fire stratagem is to ensure somehow that your hearers are physically uncomfortable. If it is a warm day, see that the windows are kept closed, for poor ventilation lulls people into thinking (but not about what you are saying). Don't forget to arrange for strong lights in the audience's eyes. This is the same device used so successfully in the third degree. Again, have the folding chairs wedged so closely together that there is no room between persons. Don't ask those in the rear of the room to come forward—it's vital to keep the audience scattered.

Insult your listeners. Either explain theories with which they are already familiar or tell them they wouldn't understand if you did explain. After all, they can always read your book if they want real enlightenment.

Do not exert your lung power. If you wish to succeed, speak so that the man in the back row wishes he were in the front row, the man in the front row wishes he were on the platform, and the man in the middle of the room wishes he were back home. This is known as complete coverage.

If you know you have only five minutes left, triple your rate and get in every word. Don't lose a single sentence.

When the speech is over, forget it. Don't recall audience reaction during the speech. It is nobody's concern, except possibly your own, whether you made yourself clear or whether you persuaded anyone.

THE SPEECH

Use long and involved sentences throughout. Join clauses with *and, but,* and *however* frequently. If your sentences run about fourteen words in length, you risk becoming downright perspicuous. Shape them into rounded periods like those of Edmund Burke and William Pitt. A sentence is scarcely worth uttering if it is less than 150 words long.

Stretch out the speech. Repeat your points. Present the same idea in any number of different ways. Then backtrack and start all over. Never organize your speech—it's too confining.

Avoid humor like the plague. If you tell stories or anecdotes or capitalize on amusing incidents of the meeting, you will be marked as an unlearned and unscientific person. Try to stupefy. Look dull and act the part. (It may take less effort than you think.)

Whenever possible use anticlimactic order. For making a reputation, there is nothing like letting down your listeners. Aristotle says that a speech should have a beginning, a middle, and an end. He was right about the the first two parts, but the best poor speeches really have no end. Those who unload them just go on and on. The past masters cultivate the false, or pseudo, conclusion. You, too, can temporarily arouse your audience with such phrases as "In conclusion," "To summarize," "To conclude briefly," "Let me now restate," "I want again to recapitulate."

Don't leave any time for questions or discussion at the end of the talk. But if you *are* caught unawares, give one or two curt, flippant replies and sit down. Sharp controversy, don't forget, becomes a speaker, and if anyone should disagree with you the

weapons to use are sarcasm, disregard of the main point, argument *ad hominem,* and some ill-natured questions of your own in return.

The best-known speakers have become personalities. Audiences seldom remember what they said. The moral for speakers on scientific subjects is: Let your audience remember you, the speaker, and not your speech.

The rules for poor speaking are simple. The inherent good character of the speaker or his education and experience have little connection with them. The classical concept of the good orator being the *good* man skilled in speaking needs to be re-examined. A 1948 version is better: The poor speaker is the *inadequate* man with nothing to say who nevertheless can painfully consume 30–60 minutes of an audience's time without profit and without the slightest qualm of conscience.

Don't begin now or later to look at any books on speech organization or delivery. You will regret it if you seek advice on how to improve your speaking. If anyone suggests that you have a recording made of your voice, shun the idea. You will be disillusioned and may even become so upset as to want to do something about improving your voice—always a dangerous symptom of incipient good speaking.

FINAL SUGGESTIONS FOR POOR SPEAKING

Do not read:

ANDERSON, V. A. *Training the Speaking Voice.* New York: Oxford, 1942.

BRYANT, D., and WALLACE, K. R. *Fundamentals of Public Speaking.* New York: Appleton-Century, 1947.

FLESCH, R. *The Art of Plain Talk.* New York: Harper, 1946.

MONROE, A. H. *Principles and Types of Speech.* (Brief ed.) Chicago: Scott, Foresman, 1945.

OVERSTREET, H. A. *Influencing Human Behavior.* New York: Norton, 1925.

SARETT, L., and FOSTER, W. T. *Basic Principles of Speech.* Boston: Houghton Mifflin, 1946.

WINANS, J. A. *Public Speaking.* New York: Century, 1915.

TEXTURE OF SNOW

We had forgotten the texture of snow . . .
only the children remembered,
welcoming it back
with outstretched hands and hearts.
Slowly it fell at first,
sifting, drifting to earth,
lighter than thistledown, feathery,
star after white star . . .
then it fell heavily,
closing the sky,
forming whirlpools of white cotton.
We had forgotten its reassuring warmth,
burning the cheeks, the palms,
but the children remembered;
they remembered, too, the soft snow-fur,
hugging it to them, a round Angora kitten—
caressing it, stroking it . . .

MAE WINKLER GOODMAN

IMPROVED METHODS OF TRANSPORT AND THEIR SIGNIFICANCE*

THOMAS H. MACDONALD

Mr. MacDonald received his B.C.E. from Iowa State College in 1904. In 1929 his alma mater awarded him an honorary degree of doctor of engineering and the Marston Medal for achievement in engineering. He has been commissioner of public roads in charge of the Public Roads Administration of F.W.A. since 1939, and received the Medal of Merit for outstanding services during World War II.

TRANSPORTATION rates its own high and secure place in the sun. Although it is not in itself the objective, it is the most potent tool for human accomplishment. There is far too great blindness to this definitive truth.

Economists have in a high degree predicated their major conclusions as to current human affairs and trends of future progress upon two considerations—production and consumption. They have been inclined to place little emphasis upon the effects of transportation on the existing status or the actual attainment of the latent potentials inherent within these two elements.

Humanists measure their conclusions by other considerations, such as capacity for self-government, proficiency in the arts and sciences, and opportunity to develop mental discipline and to advance the ethical conduct of the individual. These men of good will recognize the importance of environment in shaping the life of the individual, but they focus their ideas too closely. Back of the immediate surroundings, one of the major elements in the creation and perpetuation of these conditions, good and bad, is transportation.

If these criticisms are reasonably fair, how much greater, then, must be the lack of understanding of the ruling influence of transportation on the part of the general public. Such criticisms reflect adversely upon the colleges and universities because of their failure to include in their curricula the courses that will give the needed academic training for public leadership in the transportation field. They reflect adversely upon all of us who are connected with the many phases of transportation, because of as yet too-limited research of the character and scope needed to uncover the knowledge upon which to found sound laws and administrative policies.

It is high time that we recognize and accept the fact of the profound influence exercised by the kinds, the availability, and the characteristics of transportation upon human affairs. These factors have been potent in the past and will be increasingly more determinative of the future trends which affect the daily life of the individual and the many currents of national and international relations.

In our thinking, certainly transportation with its essential concomitant, terminal facilities, including storage, must be accorded equal weight with production, consumption, and social relations in their combined determination of human economy and social trends.

The inherent characteristics of each method of transportation as it has developed have exerted a ruling influence in creating the present pattern of agricultural and industrial development and consequent population distribution. Certainly the availability of natural resources, including fertile soils, determined what has been done *in toto,* but the methods and costs of transportation have fixed the pattern of the physical national structure as it exists today.

Beginning with the earliest form, water transport (which in the fifteenth century

* From a paper presented at a symposium on "Improved Methods of Transportation and their Import" at the Princeton University Bicentennial Conference on Engineering and Human Affairs, October 2-4, 1946.

opened the New World to exploitation and settlement by European countries) fixed the location of our great population centers. There are only nine American cities of more than 200,000 population that are not served by navigable water.

These foci of water transport became the natural origins and termini of rail lines to serve inland areas and to integrate them internally and transcontinentally. The development of rail transport radially from established ports brought this service to large inland areas. Communities dependent upon the horse and ox for local transport power developed along the rail lines at distances fixed primarily by the limitations of animal travel. At important convergings of railroad routes, communities gradually grew into cities. Their location was largely dependent upon topography, itself a determiner of rail-line location. They became the gateways from which supplies were distributed radially and to which the products of the land flowed for processing or for shipment beyond.

More recently, air transport has demonstrated its capacity for relatively light net loads to reduce the time-distance ratio to a fraction of that previously existing. The whole world is brought into physical contact measured by hours in place of days and weeks. Major lines of air transport are dependent for sustaining income upon the already established metropolitan areas and intercommunication between these.

The result of the development of water and rail transport, particularly in combination, has been to build population concentrations that, until about 1900, were compact in form and thus occupied limited land areas.

With minor exception, all three types of transport—rail, water, and air—are predicated upon regimentation of the individual. No matter how commonplace or how luxurious his personal accommodations, he becomes one of a group subject to the discipline of the fixed schedule for departure and arrival over a fixed route. Once he adjusts his personal convenience to the schedule, he normally enjoys a fast, comfortable, and safe trip. Except for suburban train service, these three types are chiefly used for passengers over relatively long distances and take on the aspect of state-wide, nation-wide, or international movement.

The transport of commodities, livestock, and all articles of commerce at present does not fall into as easily defined groups. The bulk of the tonnage moved in carload and larger units goes to the railroads and to the waterways. The average distance of the movement is relatively long. There is, however, in the aggregate a very large movement of goods of commerce and types of articles such as mail, parcel post, perishable foods, and other items of relatively small bulk and weight and relatively high value that is divided among all the types of transport.

HIGHWAY transport in its major uses is the antithesis of water, rail, and air transport. Its growth in the past two decades has been nothing less than fantastic, yet none of its early pioneers who are honest lay any claim to foreseeing its present dimensions. There was celebrated last year at Detroit the fiftieth anniversary of the automotive industry, but the early years gave little indication of the stature this type of transport was destined to acquire.

Between 1921 and 1941, the number of motor vehicles in use increased threefold, and the annual use of the average vehicle, measured by miles operated, doubled. Thus in the two decades prior to World War II, the motor-vehicle mileage increased 600 percent. After deducting trucks and busses, the state with the minimum ownership of passenger automobiles in 1941 had one car for every 9.9 persons, and the state with the maximum ownership had one car for every 2.6 persons. The average for the nation was one car for every 4.5 persons. Each car traveled in 1941 an average distance of 9,285 miles. The passenger-car capacity was thus equal to the moving of the whole population of the nation simultaneously. Although the number of cars in service was reduced during the war years, the prior highest level will be regained rapidly.

These facts are perhaps indicative of the over-all growth of highway transport, but carry little illumination as to the intensity of its impact upon existing conditions and its potency to effect changes.

The growth of industry brought with it a constantly increasing urban population and concentrated more people in compact areas. The typical city developed without much planning and without an over-all conception as to the form that might have a chance for survival. The stage was being set for a movement so spectacular it has been well named "the explosion of the cities." The motor vehicle did not create this situation. The blighted and slum areas had been slowly developing until now the estimate is made by Commissioner W. E. Reynolds that "the health, safety, and morals of some 25 millions of Americans are being seriously affected by blighted areas." Rather than creating these conditions, highway transport has become one of the basic tools to remedy them, to shape the pattern of daily life, and to make possible a new urban culture. There are 140 urban areas of 50,000 or more, which aggregate about one-half of our whole population.

There is a wide variation in the pattern of the central city and its satellite communities, but the problems of both are common to those of other metropolitan areas. Actually, some of these metropolitan areas are approaching the autonomous quality of the old Grecian city-states. Los Angeles, for example, has an actual corporate area of 452 square miles, but the urban area spreads over 600 square miles, equal to more than one-half the area of the state of Rhode Island. Within this urban area there are 1.5 million people and 30 cities of 5,000 or more population. For the larger urban areas highway transport is being used as a tool to effect—although as yet in the early stages—city redevelopment, abolishment of slum areas, opening of parks and parkways and suburban communities of far superior living facilities, with the minimum of time required to reach place of employment. There are some 2,042 cities with more than 5,000 people within their corporate limits, and the concentric bands of urban population widely overflow the corporate limits. Statistics based on the 1940 census, show that 56.5 percent of the total population live in urban surroundings and are very largely dependent upon highway transport for their daily movements and services.

Probably the most significant service in its long-range effect is the change made possible by the school bus. The little red schoolhouse is being rapidly replaced by the consolidated elementary and high schools. The relative opportunities of these two need no comment.

Rural free mail delivery started with horse-drawn vehicles, but the service has been greatly augmented by the speed and capacity of the motor vehicle, plus better roads.

Traveling libraries and other potentials have already contributed, and in the future will in an increasing degree contribute, to rural life. Opportunities for recreation, social contact, or education are now quite generally shared by the farmer, and these will be extended by reason of the rural road-improvement program now under way.

War production required many new plants employing astounding numbers of people who were dependent upon highway transport. The Willow Run and Chrysler plants at Detroit and many of the Chicago plants were built away from the hearts of the cities in locations where land was available quickly and at lower relative costs. Here, revolutionary ideas were put into practice because highway transport was available. Industrial plants were located in the areas where labor of the required skills and in sufficient numbers was established. Highway transport became a part of the assembly line. All kinds of raw materials were moved to plants over the highways, and units built in plants situated miles apart were brought together for assembly over the highways. There were numerous instances of almost fantastic procedures. For example, wings for bomber assembly at Tulsa, Oklahoma, were built at Willow Run.

One industrial organization during the war had four plants, located in three cities. It hauled, by means of motor trucks, forgings from plant A to plant B, 100 miles away, for heat treating; then hauled the heat-treated forgings another 100 miles to plant C for machining; and finally to plant D, 15 miles away, for assembly into finished unit. The prime contractor in this instance had an assembly line of approximately 215 miles between the forging plant and final assembly.

The hauling on highways in normal peacetime is shown in Table 1.

TABLE 1

COMMODITIES TRANSPORTED ON RURAL HIGHWAYS

ITEM	MASSACHUSETTS 1938	CALIFORNIA 1936–37	WYOMING 1936–37	43 STATES—SURVEY YRS. 1936–39
	Percent	Percent	Percent	Percent
Products of agriculture	8.05	16.96	17.17	12.8
Animals and products	12.37	9.93	10.00	13.2
Products of mines	4.71	2.85	12.08	6.4
Products of forests	3.31	3.80	4.01	5.2
Manufactures and miscellaneous	62.03	66.46	56.74	53.5
Mixed freight	9.53			8.9
Total	100.00	100.00	100.00	100.0

The specialized equipment available for transport over the highways of products difficult to handle has been particularly marked in the growth of the milk industry, contributing alike to farm income and the provisioning of the cities with better-quality foods. The consumption of milk in Washington, D. C., in 1925 was 40,850 tons; in 1945 it had increased to 216,290 tons—an increase of more than five times the earlier figure in a two-decade period during which the population doubled. This milk is produced largely in the near-by areas of Maryland and Virginia. The fine dairy farms with their sanitary buildings and highbred dairy cattle have become perhaps the most pleasing elements of the rural landscape in the vicinity of Washington.

THERE is far too great emphasis upon the element of competition between the available types of transportation, and far too little accent upon the degree to which each type is necessary and supplementary to the others. Each type of transportation is supreme in the service it can best render. This does not mean necessarily the cheapest service. As a general rule, the public has evidenced its willingness to pay a premium for time-saving, but this has decided limits. Since, ordinarily, water, rail, and air transport cannot perform a complete service—that is, from origin to destination—highway transport must supplement each one. Highway service is chiefly important in the short-haul field. In the transportation of persons, the figures in Table 2 are indicative.

TABLE 2

PASSENGER TRIPS—AVERAGE LENGTH

	MILES	YEARS
Rail	82	1936–40
Air	525	1946
Private motor vehicles	14.6	1937–38
Bus—intercity	30–40	1941–44

Studies of the use of the passenger car reflect in a major degree the repetitive daily travel of the average individual and his family within the environs of his own community. Eighty-five percent of all individual trips of the passenger car are within a 20-mile radius. Focusing this more closely upon the use of passenger cars in four cities of different population, origin-and-destination studies of 1944–45 (Table 3) show within what narrow limits passenger cars operate.

TABLE 3

CITY	POPULATION (METROPOLITAN AREA)	TRIPS ENTIRELY WITHIN CITY	ALL TRIPS—TRANSCITY, INTO AND OUT OF, AND WITHIN THE AREA
		Miles	Miles
Denver, Colo.	361,100	3.12	3.67
Fort Wayne, Ind.	120,000	2.07	2.61
Greenville, S. C.	75,000	1.47	1.81
Spartanburg, S. C.	45,000	1.37	1.98

In the field of truck use there seems to be little evidence to show that distance in itself is a determining factor in competition between rail and highway transportation, although trucks are at present little used for transcontinental hauling. Trucks are used, however, in regular hauling for distances of 1,000–1,500 miles. The nature of the commodity to be moved, the particular conditions surrounding the shipment, and the importance of the time element are more significant than distance in the shipper's choice of mode of transportation.

Fruit and vegetables are hauled by truck to New York City from states as far south as Florida and as close as New Jersey. Furniture is conveniently moved by van, principally because of the elimination of the need for crating. Once loaded, the van may traverse a few blocks or, as readily, as many states. Livestock movement by truck is increasing, with trips often covering substantial distances, for truck movement is quicker and loss of weight of the animals is reduced. In Ohio large quantities of coal are hauled from the southern Ohio fields to Columbus and other cities by truck. Here the product of the small mines tunneled into the hillside by one or two miners is loaded onto light trucks or semitrailers and hauled directly to the consumer. In areas of large-scale operations, however, in which loading facilities are designed for use of railroad cars, shipment of the same product by rail over the same or even much shorter distances would be the rule. Milk moves into New York City from near-by farms and likewise from the Adirondack milkshed by highway, now that suitable truck bodies are available, with the obvious advantages of its receipt in bulk rather than in cans. Many other examples could be cited in which the product to be moved and the peculiar advantages of truck movement direct from producer or shipper to consumer or processor are of far greater significance than distance alone.

Another factor in actual or potential competition between rail and highway, the importance of which should be investigated as opportunity offers, is the degree in which truck haul between points served by rail is in reality not competition. Undoubtedly, much Southern fruit is sold in Northern markets because it can be brought in quickly and with little handling by truck, when such shipment by rail would not have been practicable and would not have been made. In other words, is it not possible that the truck has developed markets that otherwise never would have existed? Probably few trucks are actually competing with the freight trains they pass.

The idea that trucks compete in tonnage hauling with freight trains becomes somewhat ludicrous when it is shown that in 1941, of the 3,711,000 registered trucks, classified according to manufacturer's capacity in tons, 3,212,000, or 86.6 percent, were in the 1.5-ton-and-less class, and over half of these, in the less-than-one-ton class.

Although highway transport in the aggregate reaches tremendous proportions of passenger-miles and ton-miles, its sphere is in the local field, supplementary to, rather than competitive with, other kinds of transport. The road program is geared into this aspect and will be governed by the principles of developing urban, main rural, and secondary roads on a balanced program to serve primarily the objectives of the communities in which they are located. In the cities the most distinctive newer type of improvement will be the controlled-access road, which is designed to carry traffic rapidly and safely into and through the metropolitan areas. The main rural roads, when they reach a daily volume of 4,000 vehicles, will be considered for improvement as four-lane, divided highways. Rural secondary roads will be built to serve the travel with the quality of minimum annual maintenance cost.

The highway builders are not fumbling the problems. The state-wide planning surveys that started generally in 1934 have been continued, and the highway program reflects the actual and potential uses that the individual highways composing the different classes or groups are called upon to serve. Many new techniques have been developed. The origin-and-destination studies alone have added tremendously to our knowledge of how and why humans behave as they do on the roads. Because the motor vehicle is so closely tied to the needs and demands of the individual, it is through research into these factors that we must determine the future highway pattern and thus the ability of the motor vehicle to serve.

SALOMON'S HOUSE: A STUDY OF FRANCIS BACON

RUFUS SUTER

Dr. Suter received his Ph.D. from Harvard in 1932. From Harvard he went to the Library of Congress as a cataloguer in philosophy; while there he held a fellowship for two years in the Asiatic Division. Engaged during the war in confidential work, he is still in Washington, now with the Army Map Service.

IN THE same year that the Pilgrim Fathers landed at Plymouth Rock a group of gentlemanly sailors embarked from Peru for China. Contrary winds carried them instead to the Island of Bensalem in the South Seas. There an official having the look of one who pitied men told such a wondrous tale that it is remarkable no one has ever returned to hear more. But the fact is that even the best maps of the South Seas compiled from the aerophotography of our Army fliers show no trace of the Island of Bensalem.

The official with the compassionate look was a Father of Salomon's House. His marvelous story is of interest to us today because we are concerned about the organization and the government control of scientific research. Salomon's House was (and is, if it still exists) the perfect answer to such problems. It combined the experimental skill and the learning of the Smithsonian Institution, the Carnegie Foundation, and the Library of Congress. It had, to boot, the social prestige and awesome authority of the Church of England and the Supreme Court.

At a time when men in Europe were barely awake to the knack of using observation and experiment for decoding the cryptogram of nature, Salomon's House had built observatories and laboratories, experimental forests, zoological reserves, sanatoriums, experimental kitchens and breweries, and clinics for advice on marital problems. When the as yet sporadic revelations of science in Europe were fighting for their existence against the vested authorities, Salomon's House had developed an efficient service for communicating scientific knowledge to the public.

In the biological laboratories some discoveries were in advance of the aims of our investigators today. The gap between the organic and the inorganic was fully bridged. Plants were grown from synthetic seeds. Living insects, worms, and reptiles were evolved from putrefaction. Crossbreeding was practiced in various combinations, occasionally resulting in hybrids able to reproduce their kind. The old Biblical challenge of adding a cubit to one's height had been met. Physicians could preserve life when vital organs had been destroyed.

In physical laboratories many natural phenomena were reproduced artificially: for instance, metals and mineral baths, electrical storms, rain, and snow. Not only were telescopes and microscopes in use, but also instruments with the opposite purpose of making near things seem far and large things seem small. There were devices for magnifying sound. All sensory phenomena—the perceptible properties of our physical environment—were studied empirically: heat, light, color—even smell and taste, neglected by modern physicists.

Like us, the Bensalemite scientists had built air-conditioned rooms, but they used them for therapeutic purposes.

If we glance at the Constitution of Salomon's House we find a statement of objectives worthy of being inscribed above the door of the Rockefeller Foundation: "Our aim is the knowledge of causes and secret motions of things; and the enlarging of the bounds of human empire, to the effecting of all things possible." In modern language: "Our purpose is to discover natural laws and to apply this knowledge to engineering, medi-

cine, agriculture, eugenics, etc., for the benefit of the human race."

Membership, according to the Constitution, was divided into classes of Fellows with somewhat bizarre titles, but with nonetheless recognizable duties. Several groups of Fellows were occupied with gathering data. The Merchants of Light made periodic trips to foreign lands to bring back reports about scientific achievements. The Mystery-men and Pioneers gleaned accounts of experiments from all the bulletins, magazines, and books they could lay their hands upon, and supervised original experiments. Other classes of Fellows, such as the Compilers, were engaged in the clerical duty of elaborating these data into synoptic tables. The Lamps pointed out new experiments hinted at in the records of their colleagues. The Dowry-men, or Benefactors, suggested implied practical applications. The Interpreters of Nature studied the huge synoptic tables for the purpose of codifying natural laws and axioms.

A point to be noted in the Constitution is that the officials—the Fellows—were never concerned with the spadework of observing and experimenting. This much of the aristocratic prejudice of the scholars of classical Greece and China against working with the hands persisted in Bensalem. The august Fellows, in their caps and gowns and seated in their armchairs, either composed and edited documents or occupied themselves with the high points of broad administration. They suggest lawyers or learned scribes rather than laboratory scientists. Actual experiment and observation were left to plodding hermits under orders, who lived all their lives on the mountaintops or in caves: the observatories and laboratories.

If we have become suspicious that this account of Salomon's House is romance, we should be reminded that truth is stranger than fiction, for the romancer is not H. G. Wells but Sir Francis Bacon, Lord Verulam, author of the famous *Essays* many of us read and memorized in high school. We have been retelling parts of Bacon's posthumously published and unfinished novel, *The New Atlantis,* written at the dawn of modern science. In this fragment Bacon visualized the specialization and empirical method that were to characterize the growth of science through the next three centuries. This actual feat of Bacon's is, in its way, as incredible as some of the wonders of Salomon's House.

We could mention other fascinating points in *The New Atlantis:* for instance, that the Old Atlantis, alluded to in Plato's dialogue, the *Timaeus,* was America. Or we could mention Bacon's wholehearted pique at the Chinese for the prohibition against Europeans entering their kingdom. Here we have a sidelight on what must have been a real sentiment in English official circles. *The New Atlantis* was written toward the end of the Ming Dynasty, when English mariners were trying hard and unsuccessfully to gain access to Chinese ports.

But rather than entertain ourselves with more tales from a secondhand source, let us visit Bensalem to learn whether the precise methods of Salomon's House in its fecund inquiries may not be available to foreigners. To be sure, we could get the same information from the second half of Bacon's *Novum Organum,* but this interminable series of aphorisms is an ordeal to read whether in the original Latin or in translation. The magnificently attired Father, whom Bacon described as having the look of one who pities men, can give us the knowledge we seek in more palatable form.

Our guide, accordingly, introduces us to the chief of the Heat Division in the Bensalem National Physics Laboratory. Fruitful inquiry into the law of heat [that worthy informs us] depends, like the study of any natural phenomenon, upon the distribution of data into three files. The first of these is called *The Table of Essence and Presence.* A catalogue of the cases where the phenomenon under examination occurs, it is, of course, in our Heat Laboratory a file of as many and as various instances where heat occurs as are ascertainable by us and our field agencies.

In a modern Kardex he shows us thousands of entries, among them "Rays of the

sun," "hot vapors," "flame," "wool," "bruised vegetables confined," "animal heat," and "dung."

The second file [continues the chief] is called *The Table of Absence in Proximity.* It is a catalogue of cases where the phenomenon being studied is absent, but where the cases otherwise resemble those in the first file.

He pulls out a drawer of this second Kardex and shows us that the cards not only have their own proper legends, but also an elaborate cross-index system. For instance, on the card marked "Rays of the moon, stars, and comets: *no heat,*" is a cross reference from "Rays of the sun." On the card marked "Vapors in their natural state: *no heat*" is a cross reference from "Hot vapors (i.e., vapors newly exhaled from, or associated with, hot bodies or compressed)." In short, only those instances of the absence of heat are recorded that show more or less similarity to the instances where heat is present. The reason for this limitation is practical. To file all cases of no-heat would require too many boxes.

We are puzzled, however, by certain cards in this second file: those bearing the blunt legend, "No negative instance known." Each has a cross reference, for example, from "dung," "animal heat," or "flame."

For certain types of phenomena [explains the chief] where heat occurs, no comparable phenomenon has been found without heat. The very fact of this deficiency is significant. Hence the "zeros," as it were, in the file of *Absence in Proximity,* indicating that with such types of phenomenon there is no case of absence in proximity.

The third file impresses us as more complicated than the other two. It is called [says the chief] *The Table of Degrees,* or *of Comparison,* and it is a catalogue of the comparative magnitudes with which a phenomenon is exhibited. Heat, of course, exhibits itself in various degrees of intensity. Thus, in our *Degrees* file we record the cases where heat occurs according to a system of ratings: 1, 2, 3, etc., indicating the relative intensity of the heat.

He pulls out a drawer at random and shows us the following series, where the larger the rating number the more intense the heat:

Ignis fatuus (Jack-o'-lanterns)	1 or less
Sparklings from the sweat of animals	1 or less
Spirit of wine	1 or less
Flame from vegetable matter; e.g., dried leaves	about 2
Flame from hair	about 2
Flame from wood (small faggots with little pitch)	3
Flame from wood (trunks of trees)	4 or more
Flame from oily substances; e.g., wax	5
Flame from pitch	6
Flame from sulphur	7
Flame from certain chemical compounds; e.g., gunpowder	8
Flame from certain imperfect metals	9
Lightning	10

In another drawer he shows us that "flame (quiescent)" is rated at 1; whereas "Flame subjected to motion, e.g., blown by bellows or lashed in a wind," is 2 or more. We observe also that this thermal rating of certain metals is increased when the metal is subjected to the motion of pounding (as the anvil under the blacksmith's hammer). It is clear to us, on the other hand, that thermal ratings in general decrease as motion is checked.

A subtle part of our technique [says the chief], having a direct relation to the efficiency of our filing system, is the matter of Prerogative Instances. Politeness may have prevented you from making the obvious criticism that if our selection of instances for our files is wholesale, if it is merely a patient accumulation of all the cases of heat and of no-heat that our Merchants of Light, Pioneers, and other field agencies, pick up, our files will be overcrowded. The fact is, we try as rigorously as possible to limit our selection of data to Prerogative Instances. Our highest-paid research workers are those who have the knack of ferreting out Prerogative Instances. There are dozens of kinds. We have them analytically catalogued in another Kardex. But I shall explain only one or two.

You have already perceived [he goes on] that we record in our File II (*Table of Absence in Proximity*) only instances that have cross references from our File I (*Table of Essence and Presence*). That is to say, we

file only instances of no-heat that resemble in other respects instances of heat. Thus, what we file are all, more or less, Prerogative Instances. To speak with strict theoretical accuracy, however, a Prerogative Instance of no-heat would be an instance that had literally every feature in common with instances where heat occurs, except one, namely, the feature that constitutes the law of heat, the laying bare of which is the aim of our technique. Such strict Prerogative Instances are exceedingly rare, and one alone, if it could be found, would theoretically be enough to solve our problem.

You also commented especially about our legend [he continues] "No negative instance known" in our *Table of Absence in Proximity;* and you acutely observed the cross references from certain cards in our *Table of Essence and Presence.* These legends lend an especial dignity to the cases they are cross references *from.* Such instances, in File I, are more or less Prerogative. Again, however, to speak with strict accuracy, a Prerogative Instance of the presence of heat would be a case that had literally no feature in common with any situation where heat is wanting. In such a theoretically perfect Prerogative Instance of heat, the entire remaining complex of features, other than the heat itself, would be unambiguously the law of heat. But, in the absence of such perfect cases, we must rely upon the hundreds and thousands of cases approximating more or less to Prerogative Instances. I could go on for hours explaining the various kinds of Prerogative Instances: for example, what we call the Prerogative Instances of the Door or Gate, made available to us by telescopes, microscopes, and other instruments for strengthening the power of the senses. But I have explained enough to make clear to you the "empirical" part of our empiricoinductive method.

Here the chief hesitates and gives us a knowing wink as if he expects us to understand that these Prerogative Instances are the most delightfully clever inspiration in the whole history of Bensalem.

All that remains to be elucidated [he says] is the "inductive" part of our empiricoinductive technique. This can be done in a few words. It is a mechanical operation. Our office force just runs through our files crossing out every item that appears in association with heat in Kardex I and also in association with no-heat in Kardex II, and that fails to dovetail with any of the graded series in Kardex III. The few remaining items, after this wholesale deletion, automatically are those that are present always when heat is present, and absent always when heat is absent, and that bear a correlation with increases and decreases in heat intensity. They constitute the law of heat. Q.E.D.

Of course we are curious to know what results the Heat Division of the Bensalem National Physics Laboratory has achieved in the application of this method. The chief informs us that after many years of research the staff has arrived at a provisional formulation of the law of heat, called "an indulgence to the understanding," or "a first vintage." We are confident [he says] that with progressive enlargements and revisions of our files we shall approximate more and more closely to an adequate formulation. At present our formulation stands as follows:

> Heat is a violent motion, expansive in all directions but especially upwards; restrained; and acting upon the smaller particles of bodies.

It is characteristic of our scientific outlook [the chief adds] that every law we unravel we regard as not only an insight of pure or fundamental science, but as also a practical instruction of how, as engineers, we can control and direct nature. Thus, our provisional formulation of the law of heat may also be stated:

> If in any natural body you can excite a dilating or expanding motion, and can so repress this motion and turn it back upon itself that the dilation shall not proceed equably, but have its way in one part and be counteracted in another, you will undoubtedly generate heat.

AND so we end our interview with the Fellow of Salomon's House. Let us consider again, however, that what we learned from the chief of the Heat Division of the Bensalem National Physics Laboratory may be read in the second half of the *Novum Organum,* by Francis Bacon (minus some twentieth-century streamlining). Bacon's

work, however troublesome it may be to read, whether in the original Latin or in the Ellis and Spedding translation, belongs on any worth-while list of scientific classics, because it is the first attempt to formulate the empiricoinductive method.

Its very title is significant: *Novum Organum*, "The New Instrument," suggesting the supplanting of the *Organum*, or orthodox "Instrument," by which the logical writings of Aristotle were known, and which was the bible of all the college professors and preachers against whom the founding fathers of modern science struggled. Bacon wrote in the age when men like Kepler, Galileo, Gilbert, Torricelli, Pascal, and Boyle were beginning to use an empiricoinductive method. It is one of the striking anomalies of history that the original formulation of this method came from a lawyer rather than from a physicist or mathematician. He was relatively unqualified for the task. Suspicious of Galileo's telescopic astronomy and contemptuous of the studies of magnetism by Gilbert, his personal physician, he was inept in his own experiments, had little if any first-hand acquaintance with the telescope and microscope, invented—and so spectacular—in his lifetime, and was incorrect with amazing fidelity in his interpretation of specific natural phenomena. He did not even accept the Copernican astronomy. One passage in the *Novum Organum* hints that he was blithely unaware of Galileo's refutation, at the Leaning Tower of Pisa, of the Aristotelian dogma that the speed of a freely falling body is a function of its weight.

One must stand in awe, nevertheless, of a legal talent able to direct its energies as happily as it did toward the codification of the empiricoinductive method. The doctrine of Prerogative Instances is a brilliant anticipation of John Stuart Mill's treatment of induction in the mid-nineteenth century—a treatment sometimes regarded as the definitive discussion in the English language.

Bacon's ever-present shortcoming is the overemphasis on the documentary, or filing, side. There is too much to-do about the scribe and not enough about the laboratory technician. But this is a weakness to be expected from a lawyer and from the Lord High Chancellor of Great Britain. A graver criticism is that Bacon concentrated off-center in identifying the scientific method with a technique of inventing hypotheses, the "hunches" scientists have of what may be a law. For the results of the Herculean labors of accumulating and manipulating the vast synoptic tables of Salomon's House are, after all, hypotheses. The central task of the scientist, in reality, is the empirical testing of the hypotheses. It is upon this empirical testing that all the ingenuity and exactitude of our modern scientific devices and the inventive genius and resourcefulness of our scientific technicians are brought to bear; and it is by this empirical testing that a hypothesis either is discarded or in time is raised to the level of a law.

MANUSCRIPT TRANSLATIONS FROM THE RUSSIAN

All geologic publications. The Committee on Russian Literature of the Geological Society of America has begun the compilation of a list of translations from Russian into English of geologic papers and books; that is, a list of translations extant in manuscript form in the United States. Please send information as to any such translations to the committee chairman or to Geological Society headquarters. The word *geologic* is used here in its widest sense—from geophysics to paleontology.

Current geologic publications. If information about translations of recent publications is received soon enough, it will be included in forthcoming volumes of the *Bibliography and Index of Geology Exclusive of North America* and the *Bibliography of Economic Geology*, subject to permission from proprietors of translations.

Ronald K. DeFord, Chairman
Box 1814, Midland, Texas.

SCIENCE ON THE MARCH

INTERNATIONAL RECOVERY IN SEISMOLOGY

INTERNATIONAL cooperation is essential to some departments of seismology. Investigation of the interior of the earth from seismic data, reliable statistics of earthquakes, study of earthquake geography—all require a widely distributed network of seismological stations. It is gratifying that the world-wide seismological program is now vigorously recovering from the disasters and interruptions of World War II. The following account corresponds to information available at Pasadena in September 1947. It does not include projects for the restoration or improvement of old stations or the establishment of new ones.

Establishment of an international seismological association was followed by publication of collated data; these were issued from the international headquarters at Strassburg and cover the years 1904–8. At this point the work was interrupted by World War I; many stations were discontinued, temporarily or permanently.

After World War I, international headquarters remained at Strasbourg, under French administration; but an international seismological summary was now published from Oxford, under the direction of H. H. Turner, who had already prepared a series of similar reports for the British Association. This summary in its official status begins for the year 1918. It has never lapsed, but has fallen more and more in arrears. During World War II it was kept up largely by the devoted efforts of Miss Ethel Bellamy. She has now retired from the work, which has been transferred from Oxford to Kew Observatory. The latest issue of the *International Seismological Summary* covers the quarter January–March 1936.

An active service of collecting and collating preliminary reports from all stations willing to cooperate, and distributing the results promptly, was developed at Strasbourg by E. Rothé, and continued there until 1940. Since the war, and following the death of Rothé, the same work has been resumed and vigorously prosecuted by his son J. P. Rothé, assisted by E. Peterschmitt and others.

Turning now to the activities of groups of stations and of individual stations, we find little change in the stations associated with, or sponsored by, the U. S. Coast and Geodetic Survey. The latest report lists twenty-eight of these. Those outside continental United States are: Balboa Heights, Canal Zone; Bermuda; College (University of Alaska, near Fairbanks); Honolulu; Huancayo, Peru (hitherto under the Carnegie Institution of Washington, but now being transferred to Peruvian administration); San Juan, Puerto Rico; and Montezuma, Chile (Smithsonian Institution). Seismograms for these stations are analyzed at Washington. As only a small staff is available, the reports are retarded; the latest available at this time (September 1947) is for April–June 1944.

During World War II, by arrangement with the Danish authorities, reports for the two Greenland stations at Ivigtut and Scoresbysund were included with the U.S.C.G.S. group; the records and reporting have now been retransferred to Copenhagen.

Delay in publication of full data for American stations is partly compensated for by preliminary determinations of epicenters and origin times for the larger shocks, circulated promptly by post card. Readings for these and other shocks are included in the preliminary bulletins of the Jesuit Seismological Association, issued from its headquarters at St. Louis University. This organization includes most of the remaining stations in the United States, and some overseas; nearly all issue individual reports, either directly or through St. Louis.

Reports are issued from the University of California at Berkeley for a set of seven sta-

tions in central and northern California, and at Pasadena for eight stations in southern California.

Readings for six Canadian stations are reported from the Dominion Observatory at Ottawa, and those for nine stations in Mexico from the Instituto de Geología, Universidad Nacional de México (the central station is at Tacubaya, D.F.).

All these functioned regularly through the war, and bulletins were issued with various delays; in most instances omitted or delayed reports have now been supplied. In the West Indies, a first-class station at Fort-de-France, Martinique, was discontinued during the war and has not yet resumed reporting. Several new stations established in the Caribbean area, in connection with the program of using microseisms to track hurricanes, are now reporting earthquakes.

South America has always been inadequately represented in proportion to its high seismicity. Huancayo, Peru, continues in service. Two first-class stations, at La Paz, Bolivia, and La Plata, Argentina, have resumed reporting and are in process of making up arrears in their bulletins. A new first-class station is in operation at Bogotá, Colombia. Reports from Rio de Janeiro are still some years behind date. The seismological service in Chile is concentrating its attention on the collection of reports of earthquakes as felt by persons and as causing damage; the station at Santiago is in operation, but no instrumental bulletins are issued. This is to be regretted; in a country like Chile, where large areas are sparsely populated and where many earthquakes originate at great depth (down to 300, or even occasionally 600, kilometers), accurate instrumental records are highly desirable, and noninstrumental reports alone may lead to conclusions that are entirely false.

European stations have been severely affected by the conflict. The Spanish stations suffered more during the civil war. The instruments at Tortosa were then destroyed, but have since been replaced. Current bulletins are now being received, with irregularities partly due to the vagaries of the postal service, from the long-established stations at Alicante, Almería, Barcelona, Cartuja (Granada), Málaga, and Toledo. The station at San Fernando (Cadiz) has discontinued pending removal to a more favorable location. The station at Coimbra, Portugal, issued bulletins for part of 1945, but none since. That at the University of Lisbon (Observatório Central Meteorológico do Infante D. Luiz) has enthusiastically resumed activity, issuing reports for the years beginning with 1940, supplemented by current preliminary bulletins.

Most of the French stations were able to continue through the war, although data were not generally available until afterwards. In 1940 the French University at Strasbourg was evacuated to Clermont-Ferrand, and the work of the international office was continued there under the Vichy regime. The station Strasbourg remained in nearly uninterrupted operation, even during the spring of 1945 when the battle front was passing through Alsace.

For Italy, current bulletins are being circulated from Rome (Istituto Nazionale di Geofisica), Pavia, and Florence (Firenze; Osservatorio Ximeniano). Reports for others, notably Prato, are received and relayed by Strasbourg. Under the direction of C. Morelli, the station at Trieste has survived the vicissitudes of the war, including Allied bombing, which on one occasion scored a direct hit on the Geophysical Institute, resulting in the loss of several weeks' records. At this time it is uncertain what effect the newly established free-state regime will have on the activities of this very enthusiastic and cooperative observatory.

One of the best of the German stations, at Stuttgart, has resumed reporting since 1946, under its able director, W. Hiller. Stuttgart is in the American zone of occupation, so that bulletins are received regularly at Strasbourg and in this country. Hiller has also sent out reports covering the early war period down to June 1942, after which the station was closed. The valuable auxiliary station at Messstetten is also operating; it is located in the German foreland of the Alps, where minor shocks are relatively frequent.

Stations are operating at Collmberg (near Leipzig), Jena, and Potsdam, but their data do not yet reach this country.

The troubled history of our time is reflected in the vicissitudes of one of the minor Central European stations, located in the Sudeten area of Czechoslovakia. Previous to World War I this was in Austria and reported under its Germanic name of Eger. After that war, reports were issued, first from Praha and then separately, under the then official name of Cheb. During the Hitler regime, reports were sent out as from "Eger (Sudetengau);" and now current reports edited at, and issued from, Praha are being received under the name of Cheb. Another unfortunate station has alternated between the Hungarian name of "O-gyalla" and the Czech "Stará Ďala."

Among European stations that have continued through the war or have resumed since its close may be listed Athens, Beograd (Belgrade), Bucharest, Budapest, De Bilt (near Utrecht), Helsinki, Praha, Uccle (near Brussels), Upsala, and Zurich. Others, such as Copenhagen and Sofia, have issued delayed bulletins, but are not circulating current readings.

The most remarkable recovery since the war is that of the stations in the Soviet Union. Extending from the Crimea to Vladivostok, these are an important part of the international network. The Seismological Institute of the Academy of Sciences at Moscow is now issuing a monthly bulletin, which is reasonably detailed although nominally preliminary, giving readings for twenty-three stations. These include all those in operation before World War II, with the following exceptions: Pulkovo, located at the observatory destroyed during the siege of Leningrad; Leningrad, a former minor station in the same city; Sebastopol, one of four stations in the Crimea of which the other three have been restored; and Tiflis, which is under independent administration. (The Academy of Sciences of the Georgian S.S.R. has recently issued reports giving readings for Tiflis through September 1939, but none of later date.) The list includes several new stations; notice of the latest, Ashkhabad, in Central Asia, is included with the bulletin for June 1947.

Africa is poorly represented. The stations at Algiers and Tunis are part of the active French group. Helwan (near Cairo) is in operation, with reports several years behind date. The Cape Town station, which was at first at the Astronomical Observatory and later moved to the University, has apparently been discontinued.

Fortunately, the very important station at Tananarive (Madagascar) continued through the war, and is now reporting regularly through Strasbourg. Ksara, near Beirut (Lebanon), continues to issue excellent reports; during the war, and occasionally since, this station (like some others) has been handicapped by running out of photographic paper, leaving only less sensitive instruments, recording on smoked paper, in operation.

Bulletins for several past years, as well as current reports, have been received from Istanbul (Observatory of Istanbul-Kandilli). The group of good stations in India continued through the war period; printed and manuscript data are available at Pasadena through 1945. Disturbed conditions resulted in rather frequent loss of records. Recent correspondence indicates that the political changes in India may lead to further reorganization.

In the Netherlands East Indies the principal station at Batavia continued in operation through the war. A bulletin was recently issued giving data through 1941 for this and its two principal auxiliaries at Amboina and Medan, which were destroyed later.

There has been no word of the fate of the station at Phu-Lien near Hai-phong, Indo-China. The station at Hong Kong was discontinued in 1940.

The observatory at Manila, with all its records, was destroyed in 1945 by the Japanese soldiery, who poured gasoline on the seismograms and burned them. Of all the accumulated data at Manila, only a history of Philippine earthquakes by Father Repetti was salvaged; this has been published in the *Bulletin of the Seismological Society of America*.

At Zi-ka-wei, near Shanghai, Father Gherzi succeeded in keeping the instruments operating through the war, in the face of many difficulties, among them the inevitable lack of photographic paper. The stations at Nanking and Chiufeng (near Peiping) were put out of action by the Japanese invasion in 1937. With great effort Dr. S. P. Lee, formerly in charge at Chiufeng, succeeded in

constructing a seismograph which recorded from 1943 into 1946 at Pehpei, near Chungking.

A small instrument formerly operated at Agaña, Guam. This was destroyed, but a new station, established to record microseisms for use in tracking typhoons, has begun reporting earthquakes.

Information as to the present status of Japanese seismology was obtained by Professor Gutenberg during a recent visit. There were two rival research centers. One, the Earthquake Research Institute, connected with Tokyo Imperial University, maintained several stations in the Tokyo area for the study of local earthquakes. These have mostly survived the war and are in operation. The present director is H. Tsuya, a geologist.

The other center was at the Central Meteorological Observatory in Tokyo, which reported data for more than one hundred stations in the Japanese islands and possessions. This institution is now under the directorship of Dr. K. Wadati, a well-known seismologist. Reports reaching this country through U. S. Army channels give readings for the stations at Sapporo, Sendai, Tokyo, Yokohama, Nagoya, Osaka, Kobe, Fukuoka (sometimes spelled Hukuoka), and, of all places, Hiroshima. The many minor stations are presumably now out of action, but it may be expected that they will be reoccupied and operated when possible. Some, such as those in Formosa and Korea, are now out of Japanese hands. So far there is no news of the international latitude station at Mizusawa, where there was also a seismological installation.

Seismology in Australia is suffering from governmental economies antagonistic to scientific work. The station at Melbourne is definitely closed. No reports have been received from Adelaide since 1942. On the other hand, Perth and Sydney continue to report, and the excellent station at Riverview (near Sydney) is operating under the very productive direction of Father D. J. K. O'Connell. At Brisbane (University of Queensland), a station was initiated in 1937; sensitive Benioff instruments have been recording there since September 1943, and their data contribute much to the seismic survey of the southwestern Pacific area. Apia (Western Samoa), though equipped with less sensitive seismographs, is very favorably located; its continued reports are of great value. An improved station began recording at Suva (Fiji) in 1943, but no later reports have been issued. The valuable station at Papeete (Tahiti), which operated from 1937 to 1939, is another war casualty.

The seismological program in New Zealand, with headquarters at the Dominion Observatory, Wellington, continues to expand and improve, with emphasis on the local earthquake problems.

An interesting temporary addition to the world network is provided by the Ronne Antarctic Expedition, located at Marguerite Bay, which is reporting by radio the readings from a sensitive seismograph. This assists greatly in locating shocks in the Southern Hemisphere.

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EXPERIENCE IS THEIR LABORATORY

WHEN a newcomer to the tropics learns that the family next door lives day after day on what appears to be a submarginal diet of rice, salt, and vegetables, he is likely to wonder how its members survive. And yet there are thousands of people in Indonesia who, though apparently defying the normal rules of nutrition, enjoy the best of health.

During the past twenty years I have studied the average diet in Indonesia. I have learned that many people have acquired a practical knowledge of nutrition—a heritage of empirical knowledge that has enabled them to live in harmony with the tropics. And after studying examples of judicious feeding I believe I have found evidence that is im-

portant not only in the tropics, but everywhere that scarcity and famine are a threat.

Of course, many still eat unwisely. For example, in a village in the immediate neighborhood of a tea plantation in Java, government officials reported a child death rate of 7 percent because of xerophthalmia. Investigation showed that the older children were neglecting the younger ones, feeding them almost exclusively on white polished rice. The parents, who worked every day on the tea plantation, had no time to prepare the noon meal; and the older children did not go to the trouble to collect the vegetables that grew in abundance on their own property. This is an example of improper feeding, and there are others. Yet malnutrition is relatively rare. Through trial and error and trial again, these people and their ancestors have found their way to abundant and nutritious foods. And what they have learned through experience, specialists in nutrition are now proving true in the laboratory.

In some parts of Indonesia the population depends principally on sago, a starchy food that comes from the sago palm (*Metroxylon* sp.). After eating sago for several months people become weak and need a change. The remedy is to move to districts where they can eat an abundance of pili nut (*Canarium moluccanum* Bl.). The pili nut, known for its recuperative power, has a content of 15 percent protein, 66 percent fat, and 140 I.U. Vitamin B_1.

A densely populated area in Java furnishes another example. There, because of poor soil conditions, the farmers cultivate tapioca extensively; their diet consists almost exclusively of tapioca roots, a one-sided carbohydrate nourishment. Results of a medical investigation indicated that the population was not excessively fat. It seemed highly probable, however, that on account of this simple diet, a deficiency was bound to show up somewhere. The reason it did not occur is interesting: exhaustive investigation proved that the leaves of the tapioca, eaten daily as vegetables, contained 8 percent protein, 5,000 I.U. Vitamin A, and 50–100 I.U. Vitamin B_1. This observation is of great scientific value because it indicates the digestibility of plant proteins, alone or in combination with others. Not only from a medical but also from an economic-agricultural viewpoint, these studies were important in solving feeding problems of population groups in other parts of tropical countries. Similarly, they can be beneficial to our Western civilizations, where every year our cities face problems in feeding and nutrition.

It is no accident that on hundreds of small islands, the so-called drumstick trees (*Moringa oleifera*) are found in nearly every dooryard. Every day inhabitants of those islands use the fruits and leaves of the *Moringa oleifera* as a side dish. Chemical analysis shows that the leaves contain 8 percent protein and 2 percent fat. The drumstick tree, easy to cultivate and bearing after a few years, is an indispensable source of vegetables found by experience to have high nutritive value as compared with other plants.

Also important is the fact that every day a dish is served that includes Spanish red pepper, with its 4 percent protein, 900 I.U. Vitamin A, and 70–250 I.U. Vitamin C (ascorbic acid).

I noted another benefit of empirical knowledge in a very densely populated section of Java. The average family had just sufficient rice and vegetables, but fat from the coconut was available in large quantities. Because of widespread unemployment, protein from animals was out of reach of the population. The people had chickens, but they did not eat them, preferring to sell the eggs for high prices. The people got their protein, however, and for almost nothing. Every small farmer kept bees, which fed on the nectar of the coconut flowers. But the farmer was not seeking honey; he valued his bees for the wax and white larvae they produced. Young white larvae, fried in coconut oil, furnished plenty of animal protein. The beeswax was used in making batik cloth.

What was the reason for the apparent good health in the heavily populated sections of Java, where the population per square mile averages from 1,100 to 1,300 persons? From the greatest altitudes at which men live down to the shores of the sea, whether on clay or sand, on flooded swamp or dry ground, the entire wealth of the tropics is laid open. It is

a wealth of fruits and vegetables, which hold the dominant position in the population's diet. Here is to be found the remnant of the primitive system of collecting plants and animals from the wilds. In the beginning, only plants that were suitable for food, for medicinal uses, for ornaments, or for firewood were grown in the dooryards. This is still true today. Even though dooryard plants sometimes find other uses, they can revert at any time to their original purposes. It is estimated that these dooryards take up about 15 percent of the arable land in Java. They are usually found in compact complexes adjacent to the houses and within the villages. The dooryards are planted, as it were, in stories. The upper story consists of coconuts, mangoes, and species of *Durio, Spondias, Gnetum, Lansium, Parkia, Artocarpus,* and other similar trees. The middle story is the level of the smaller plants, such as bananas, and species of *Averrhoa, Annona,* and *Eugenia.* On the lower story are found undergrowth varieties, especially tuberous plants, including tapioca, sweet potatoes, *Coleus tuberosa,* the Zingiberaceae, such Leguminosae as *Cajanus* and *Psophocarpus,* and the ever-present and indispensable red Spanish pepper. Between these layers wind other tuberous plants belonging to the Dioscoreaceae and Cucurbitaceae families.

The botanist who strolls about an Indonesian dooryard will notice plants of many varieties. As a result of this diversity the proteins from the dooryard are of numerous kinds and of almost uniformly good quality. Thus, although the consumption of animal proteins is very small, the dooryard supplies a good substitute in plant proteins, as well as furnishing vitamins and minerals. The coconut is an adequate source of fats. Thus, the role played by the dooryard in the daily menu of Java and other islands in the tropics can hardly be overestimated. This may also be responsible for the fact that in Java, as the total amount of land owned falls off, the area of the dooryard per person increases. It is as though it were felt that, as the absolute amount of available food becomes less, more attention must be paid to the composition of the menu. Few forget their heritage of empirical knowledge.

Even though owners in some coconut districts in Java are periodically prosperous, the old remnant of "collecting agriculture," which has proved of such great value, still remains largely intact. We must not lose sight of this factor of ethnological economy, which has proved important here, as in so many other parts of the world. Investigation is still far from complete, and it is not merely for curiosity's sake that scientists must find correct answers to many questions. What is learned can help prevent famine and illness caused by food deficiencies, not only in the tropics, but also in other parts of the world.

In his production of home foodstuffs, the small tropical farmer is often able by empirical knowledge to solve his difficulties in the field of nutrition. He knows the importance of special food crops for his existence, and he has a variety of minor plants such as grow nowhere else in the world. This variety gives him a certain protection. Otherwise his food problem, incomprehensible to the uninitiated, would be much more complicated than it is; there would be more food problems and more deficiency diseases.

J. J. Ochse

Miami, Florida

BOOK REVIEWS

DANISH SCIENTIFIC HISTORY

Naturforskeren Johannes Schmidt. Øjvind Winge and Å. Vedel Tåning, Eds. 187 pp. Illus. Kr. 12.75. Gyldendalske Boghandel. Copenhagen. 1947.

IN A world where a man's work is often only too quickly forgotten, it is a pleasure to see the volume the Danes have dedicated to the late Johannes Schmidt, one of the best known of the many students of marine biology Denmark has produced. The typography is up to the usual high standard characteristic of the old Danish publishing house of Gyldendalske Boghandel. Looking through the lavishly illustrated volume, one would never guess at the economic struggle going on in the land in which it has been published.

The book is paper-bound and carries on the back a severe black-and-grey medallion showing the classical features of Johannes Schmidt; the front displays a colorful cartoon, worthy of the *New Yorker,* with a white boat (presumably the *Dana*) flying the Danish colors, sailing over an emerald-green sea and towing a series of plankton nets astern. In the sea a fantastic deep-water fish treads a dance with a huge, luminous crustacean. It is probably only in Denmark that a dignified firm would send a book of this kind out into the world with such a cover. Actually, the contents of the book live up to its apparently contradictory cover. It contains a perfectly sober and detailed account, written by his friends and colleagues, of Johannes Schmidt's life and accomplishments, but told with such a feeling for the dramatic that one keeps on reading as if it were a Dumas thriller.

After a brief account of Schmidt's family background, the book describes Schmidt's first expedition (to Siam), when he was twenty-four years old, and continues with his activities in the Danish fisheries and with his ever-expanding expeditions in search of the solution of the two-thousand-year-old problem of where the eel breeds. Schmidt's work on the eel runs, of course, as a red thread through the book, but on the side one learns here for the first time how much Schmidt accomplished along other lines which did not capture the public's fancy as did the life history of the eel. Besides being a man of unusual native ability as a biologist, Schmidt had a rare faculty for organizing the most diverse problems, from detailed studies of heredity in plants and fishes to extensive field projects dealing with the development of the food fishes in the North Atlantic and the factors influencing the migrations of these animals. His work extends, therefore, over an unusually large field and is of both a practical and a theoretical nature.

Schmidt seemed born under a lucky star: getting a well-organized expedition on its feet at an age when most men are still working for their degrees; afterward being trained by such an old fox as C. G. J. Petersen, who had the courage to hand an enormous responsibility over to a young man in his twenties (but who also had the satisfaction of watching this same young man find and solve more problems than anyone had expected). On top of this, Schmidt became at an early age the leader of the biological division of the Carlsberg Laboratory in Denmark, which gave him use of the best-equipped laboratories in the country, while at the same time he was free to continue his work on active marine expeditions. His scientific exploits were backed by the wealth of the Carlsberg Foundation, and an always sympathetic and understanding government was ready at any time to supply ships and gear for his expeditions. Last, but not least, he had the full support of numerous Danish scientists—physicists as well as biologists—who worked up the large array of data he accumulated. Without realizing it, the authors have written the

story of Denmark's exploration of the sea from the mid-nineties to Schmidt's death in 1933, with its unique cooperation between the government and private institutions and individuals, and the resultant advance in practical and scientific research.

The individual chapters in the book are concise and packed with information; it is a relief at last to find a book entirely free from padding. Each contributor has given his best in the shortest space, and his own enjoyment of his work, whether in the field or in the laboratory, permeates each account. This holds true for the first chapter—about the Siam trip—written by the now eighty-year-old Dr. Mortensen, as well as for the youngest biologist's wide-eyed account of Schmidt's final, triumphant cruise around the world, during which he "bagged" practically all the eels then known to science, thereby giving the taxonomist occasion to write an inspired paean on the classification of the eels.

One understands why Denmark wanted this tribute to be written in Danish, so that Schmidt's lifework could be more fully known to his own people. But one wishes that the authors of the various chapters could be persuaded to rewrite their accounts in English, for "to translate them" is hardly the phrase to use in connection with these men, who publish most of their papers in English. The book is one that should appeal both to the experienced fishery expert and to the beginner, and it should also find many readers outside the scientific world.

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UNIVERSAL LANGUAGE

In Search of Beauty in Music. Carl E. Seashore. xvi + 389 pp. $4.50. Ronald. New York. 1947.

Professor Seashore, an outstanding scholar in applying science to music, attempts to integrate his popular articles and research work at the University of Iowa in this book.

Seashore's approach is configurationistic. Music draws heavily on a number of basic sciences, and it has of necessity been required to await developments in fields such as physics, physiology, anatomy, anthropology, genetics, and general and experimental psychology. The remarkable progress made in these disciplines of recent years has aided immeasurably in placing musical science on a firm foundation.

Although research is still in its infancy, the growing intimate relationship between music and other sciences has been of mutual advantage and benefit. As a result, many superstitions and much fallacious thinking about music have been thrown overboard.

Applications of the sciences, especially of psychology, to music are innumerable. Methods of evaluating musical talent; methods of guidance and of measuring musical achievement; organizing a growing body of principles in educational psychology toward musical training; paving the way at all levels for principles of musical criticism; organizing the scientific description of musical tones and the means of producing them; understanding into the nature, scope, and limitations of musical hearing and appreciation; and use of performance scores for the detailed analysis and the quantifying of artistic elements in musical performance are merely a few results of the contact between music and psychology.

There is some unevenness in the material of the thirty-five chapters. This should be expected since each chapter has previously been presented in whole or in part in a variety of journals, and since the book is designed for such a heterogeneous group as advanced students of music, psychologists, music teachers, educators, professional musicians, and general readers interested in the scientific approach.

Isolationism is no longer valid even in music. The artist, teacher, critic, and listener must comprehend the gestalt—must appreciate the interrelationship of art with the physical and social sciences as a prerequisite in the search for beauty in music.

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THE U.S.D.A.

Two Blades of Grass. T. Swann Harding. xv + 352 pp. Illus. $3.50. Univ. of Oklahoma Press. Norman. 1947.

THE purpose of Harding's book is to show the money (economic and social) value of agricultural research. The four parts (Preliminaries, Achievements, Departures, and Values, with Appendix) are crammed with a profusion of information (historical, scientific, and statistical) on the forerunners, creation, development, organization, and achievements of the numerous successive units of the U. S. Department of Agriculture.

Part I (four chapters) covers How It All Started, Agricultural Science in the Patent Office, Early Research in the Department of Agriculture, and A Few Connecting Links, the last a survey of successive secretaries, of certain laws and their effects, and of a few early problems, such as Hessian fly, silk production, sugar from maize and sorgo, microscopy, etc.

Part II contains ten chapters, nine dealing with different subjects, under fanciful names: Test Tube Magic (chemistry), The Bugs Crawl Out (entomology), All Flesh is Grass (plant industry), Man Can Help to Make a Tree (forestry), A Very Grevious Murrain (animal industry), A Good Soil (soils, fertilizers, erosion), Food and Raiment (home economics), Science at the Milkpail (dairy industry), and Agricultural Engineering Triumphs (drainage, irrigation, machinery, structures). Sandwiched in the middle (chap. 10) is Nation-wide Research Subsidy, which discusses the Land-Grant College Act, the Agricultural Experiment Station Act, and the Office of Experiment Stations, and then reviews a mass of research cooperation between the Department and various state experiment stations in most of the subjects to which the preceding and following chapters are devoted.

Part III (chaps. 15–18) recites the achievements of four Bureaus formerly but not now in the Department: Something Could Be Done About It (Weather), Men on Wheels (Public Roads), Birds and Beasts (Biological Survey), and Fraud in Foods and Drugs (Food and Drug Administration). It is stated that data for the first two were furnished in manuscript form. Much of the early work on foods and drugs had been reviewed in Chapter 5, Test Tube Magic.

Part IV contains Value of Pure Research (chap. 19) and an Appendix on The Publication of Research.

The volume contains an immense quantity of valuable information, but a casual inspection shows that the subject treatments are not uniform, sequential, complete, or always accurate. Arrangement and emphasis seem to reflect writer preference rather than reader need, in spite of the commandment: "All writing is for the reader."

The peculiar lack of sequence is maddening. A chapter usually opens with several unrelated and nonchronological items and then, with little warning, drops back and starts a more detailed review. For example, first Commissioner Isaac Newton dies on page 23 and, on page 28, we learn what he did. Plant Industry opens with a casual observation by a minor scientist on retiring in 1945.

Unexplained omissions occur. The Bureau of Agricultural Economics is omitted from the chronological list of Bureaus created (p. 36), and nowhere is its valuable research discussed. Plant Industry, the largest Bureau, with next-to-longest chapter, apparently has the least complete and accurate treatment. Ignored are the basic Western forage surveys, the international economic survey of the Great Plains area, early soybean studies, the alfalfa program, weed-control research, the lifework of Dewey on fibers, of Stuart on potatoes, of Gould on peaches, etc., etc. Self-advertisers are lauded repeatedly.

The Bureau's largest Division, Cereal Crops and Diseases, is not named. No mention is made of its early and continued advocacy of Federal-state cooperation in research to save money, speed results, and ensure harmony, or its putting its entire nation-wide research program into cooperation with state and Canadian experiment stations, a policy publicly commended by two secretaries and the basis for recent Federal legislation. Omitted are its extensive cooperative breeding and production programs

on barley, corn, flax, grain sorghums, rice, and wheat (except durum), including control of destructive diseases. Outstanding leaders like Clark, Harlan, McCall, Richey, Stakman, and Warburton are not mentioned. A single wheat item (rust-resistant Thatcher) is inserted twice, once inaccurately. The international classification of wheat and barley varieties and successive five-year varietal surveys are ignored.

The Division's largest project, the gigantic cooperative barberry-eradication campaign, is not mentioned, although it destroyed more than 30,000,000 plants, discovered the production of rust hybrids in nature (200 physiologic forms), and saves the corn belt and northern Great Plains wheat crops from increasingly destructive rust epidemics, a value in the hundreds of millions of dollars.

Errors abound: in names ("Barry" for Parry, "Clerk" for Clark, "Lowe" for Loew, etc.); in dates (of two international expositions, photograph of Department staff, etc.); and in facts (statement that no Bureaus were created before 1897, etc.). The 31 pictures (16 plates) are excellent, but mostly paired and inserted without regard for subject. In conclusion, any reader who does not check and doublecheck may suffer "a very grevious" misleading.

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SURVEY PROBLEMS

Say It with Figures. Hans Zeisel. xvii + 250 pp. Illus. $3.00. Harper. New York. 1947.

THE subject of this interesting book is inadequately suggested by the title, and the subtitle, "A Manual for Statisticians in the Field of Consumer and Opinion Research," is definitely misleading. The author is concerned largely with the conceptual analysis and the problems of meaning in the treatment of survey findings, rather than with the assessment of reliability and the use of the common statistics that have been the main concern of the statistician.

As a treatment of problems of classification, summarization, and interpretation (as the three parts of the book are characterized by Paul F. Lazarsfeld in his introduction), the book is almost unique and of considerable value. Methods of presenting reasons for behavior, useful procedures for handling other types of multiple answers, the interpretation of "Don't know" answers, the use of percentages and other derivative indices, the value of cross-tabulations, and the panel technique are discussed with examples of how their application clarified some survey problem. A particularly good discussion is presented concerning how to judge indices, exemplified by a subject of high general interest, the baseball batting average, which is criticized, and a new index derived.

The book is a publication of the Bureau of Applied Social Research of Columbia University, and much of the data come from the Bureau's files. More frequent references to other surveys would increase the scope and value of the work. For example, the rotation procedure whereby the Bureau of the Census spreads its sampling variation over a number of months in a continuing panel rather than abruptly absorbing it between panels, and the coding of intensive interviews practiced by the University of Michigan Survey Research Center (and used also by the Lazarsfeld group) would be worth-while additions to the text.

In his attempts to derive meanings from data, the author is often incautious. References to sampling error are meager, and tests of significance completely neglected. Although this is recognized in the book (footnote, p. 145), conclusions are drawn without regard to reliability. For instance, a set of correlation coefficients ranging from +.35 to −.43 is referred to (p. 28) as indicating "that there is indeed a very strong affinity between certain types of [radio] programs." The author here applies a Pearsonian coefficient to fourfold tables, without even stating the number of observations, or assessing the underlying unequal frequencies as a possible spurious factor in relationship. Similarly, in the treatment of cross-tabulation, the following questions are stated (p. 170) to be "the same question in different words": "What are the factors which *determine* the size of

this proportion or its complement, the people intending to vote for a Democrat?" and "What factors *characterize* the group which intends to vote Democratic as compared with the group which intends to vote Republican?" (Italics by the reviewer.)

Such treatment makes the book dangerous for those not grounded in statistical caution, and a statistician will find a lifetime of work in determining the variances of statistics used. There are, however, techniques useful for the imaginative analysis of social data, and anyone not solely interested in the manipulation of numbers will find the discussion stimulating, and the book a valuable adjunct to the ordinary text in statistics, which is weak on this creative aspect of analysis.

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BOTH GREAT AND SMALL

A Multitude of Living Things. Lorus J. Milne and Margery J. Milne. 278 pp. Illus. $4.00. Dodd, Mead. New York. 1947.

CHARLES LAMB was wrong. He thought that a mussel, once anchored, was fixed for life, but that was disproved by Reaumur, who put one in a dish of sea water and watched it travel. Some flies make webs that capture spiders. One salamander has no lungs, breathes through its skin.

Facts like these are contained in a book whose title is almost a review of it: details of "a multitude of living things" are packed—I might almost say jammed—into it, but it is easy reading.

The king crab has his chapter, and so does the speleologist. Somewhere I have read that there are a hundred ways in which to cook an egg, but there is much more to an egg than cooking it. The writers have done a chapter on Consider the Egg, which treats of this defenseless organism, food for hungry animals, but somehow able to carry on for itself.

Rivers that no longer exist carried hardened amber from pine forests of the Baltic into the sea. Now it is mined and made into pipestems and jewelry. Don't buy an amber necklace until you have consulted this book, because there have been many imitations of this substance.

Do you know how the blind fish of Mammoth Cave became blind? The book will tell you.

There is some discussion of eyes—the eagle eye, the eyes of squirrels and of the praying mantis.

The authors tell also of hay fever caused by the microscopic scales from the wings of caddis flies, which gives an idea of the scope of the book.

And if you want gruesome romance, the exceedingly good account of the sexton beetles should suffice.

It is difficult to make comparisons, but this book reminded me of some of the works of Fabre. Read it a chapter at a time and enjoy it. There are a few—too few—photographs; those that are presented are unusually good.

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PAMPHLET

John Couch Adams and the discovery of Neptune. Sir Harold Spencer Jones, The Astronomer Royal. 43 pp., with a portrait of Adams. 75 cents. Cambridge Univ. Press. Cambridge, Eng. Macmillan. New York. 1947.

THIS astronomical *Whodunit* explains clearly the complex circumstances surrounding the discovery of the planet Neptune and gives a brief account of the controversies that followed. Sir Harold puts the reader in a position to judge for himself the parts played by Adams, Airy, Challis, Herschel, Le Verrier, and Arago. Half an hour with this delightful pamphlet will give you the inside story of a famous episode—a story that sheds vivid side lights on the human element in research.

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THROUGH PATHLESS REALMS

Rockets and Space Travel. (Rev. ed.) Willy Ley. 384 pp. Illus. $3.75. Viking. New York. 1947.

THIS book is a revised and enlarged edition of Ley's *Rockets,* 1944. Two complete chapters have been added and the original chapters revised and brought up to date. Ley's book describes the early history of rocketry in the greatest detail and interest in English. The treatment of the growth of the concept of free flight in space is treated in a scholarly manner. One, however, should not think that *Rockets and Space Travel* is just another popular book on the subject. The appendix consists of over fifty pages of straight technical information in fine print and is an excellent introduction to rocket theory.

If we go from the general to the specific, we find that Ley has grouped the illustrations at the beginning of the book, which captures interest in an excellent manner. Then follow twelve chapters on the history of rocketry, the development of rocket theory, the use of rockets in the second world war, and finally a popularized account of the application of rocketry to the space ship. The rocket enthusiasts have been talking about space ships for years, but most scientists took their enthusiasm very lightly. Now that it is evident that space travel is only a few years away and that the first nation to establish a military base on the moon will dominate the earth, the development of space flight by engineers and scientists is one of the primary objectives of research sponsored by various governments.

The thesis of space travel, and especially the construction of a terminal in space as a take-off point, is described. The discussion of the application of atomic energy to space flight (pp. 282–3) is not very good in an otherwise excellent treatment. The methods described, such as the use of soluble U-235 salts in water to produce superheated steam, or the use of U-235 on a feed wire into a combustion chamber, are not the most probable devices for a thrust drive. An enriched, high-temperature pile using a secondary fuel for the mass effects of the blast stream appears to be the best way of utilizing atomic energy in a space ship. It should be observed that atomic energy is not a prerequisite of space flight. Chemical fuels will put a ship on the moon and allow construction of the space terminal. Ley emphasizes this point.

If the reader wishes to get a modern, up-to-date, declassified survey of the position of rocketry, Ley's book should be read in conjunction with G. Edward Pendray's *The Coming Age of Rocket Power* (New York: Harper, 1945). The two books supplement each other. Most of the engineering and scientific data on rocketry is being published in the *Journal of the American Rocket Society,* with a scattering of articles in other journals, such as the *American Journal of Physics* (Seifert, 1947). The development of rocketry on a practical basis, as described by Ley and Pendray, will have as much influence on civilization as the development of atomic energy.

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EVOLUTION OF CIVILIZATION

Genetics, Medicine, and Man. H. J. Muller, C. C. Little, and Laurence H. Snyder. vii + 158 pp. Illus. $2.25. Cornell Univ. Press. Ithaca, New York. 1947.

THIS volume of 158 pages comprises the six 1945 Messenger Lectures which are given annually to provide "a course of lectures on the evolution of civilization, for the special purpose of raising the moral standard of our political, business, and social life." Dr. Muller's chapters (The Work of the Genes, The Dance of the Genes) survey in a sprightly fashion the fundamental properties and actions of the genes as they play their role in the biochemistry of life and sex. In nontechnical language, he boldly pictures the great cooperative enterprise of the germ plasm as it provides a short-term as well as a long-term adaptation of the species to its environment.

Dr. Little stresses the relation of genetics to medicine by a masterful review of research on laboratory mammals. Parental influences at the morphogenetic, emotional-endocrine, and psychosocial levels of environmental activity are summarized, with the hope that medicine will be provided with a sound biological foundation of experimental facts. His second chapter deals with integrated growth and individuality of the organism as these are related to the cancer problem.

Dr. Snyder summarizes many of the known data on human heredity with special stress on medical applications (prognosis, diagnosis, prevention of human defects). A simple exposition on the genetics of the blood antigens A, B, M, N, P, and Rh is included. His final chapter on the mutant gene in man provides a particularly clear statement of the genetic basis for human heredity, including the most recent advances on sex-linked genes.

There is an agreeable unity in the book despite its triple authorship. It must be considered as an accurate and modern presentation by these three authorities in their respective fields of genetics.

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WATER

The Story of Water Supply. F. W. Robins. x + 207 pp. Illus. $4.50. Oxford Univ. Press. London, New York, Toronto. 1946.

THE story of the human struggle and success in making water available for the almost innumerable purposes to which it is vital began with man's advent on the scene, has added chapter upon chapter through the ages, and is still being written. F. W. Robins, within the compass of this small volume, has traced the stream through the history of man's economic, political, and cultural development from the earliest times down to recent engineering projects.

Primitive man lived along rivers and other natural water sources. In order to settle in areas distant from natural supplies, he had to devise means of bringing the water where he wanted it. Mr. Robins has selected examples from early records in every corner of the earth to illustrate the mother wit and ingenuity of man in making water come to his bidding. Not only in ancient India, Mesopotamia, Egypt, Greece, and Rome were there outstanding waterworks, but also in thirty or more other countries from Abyssinia and Zanzibar to Peru and Mexico.

Little less than astounding were the variations and effectiveness of canals lined with stone or concrete; aqueducts on arches, some still in use after all these centuries; underground conduits; and pipes of lead, copper, bronze, wood, terra cotta, bamboo, etc. The rivers had been nature's water mains, and springs her pumping stations. Man had to create his own water mains and pumps of various kinds. The size and complexity of such installations varied according to the number of people to be served. In ancient Rome, with an estimated population of 1,000,000, eleven principal aqueducts brought a daily supply of 40,000,000 gallons of water to the city from distances varying up to forty miles. Canterbury Cathedral was supplied with water from springs only a mile distant. In A.D. 1160 that was quite adequate for the small community.

Many medieval towns, monasteries, friaries, and private palaces possessed elaborate indoor equipment, with spigots in kitchens and sprinkling devices for shower baths. Robins pictures graphically the various kinds of water dippers used by early man, made, as they were, of gourds, calabashes, bamboos, animal skulls, bark, leather, coconut shells, and, of course, the cupped hand, which preceded all others.

There are special chapters on "wishing wells," water divining, water sellers, and parish pumps. England is treated in such intimate detail as to make the book a Baedeker on the Roman, Anglo-Saxon, and medieval water systems in that country. We are told that there are more than 400 wishing wells in England, around many of which superstitions or actual beliefs still linger. Although Robins was skeptical of the claims

made of locating water sources by divining sticks, he was convinced of the authenticity of the method in one case.

The wide range of data garnered from his global ramblings through the literature on ancient man is documented by footnote references on nearly every page.

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THROUGH A GLASS, DARKLY

From Caligari to Hitler. Siegfried Kracauer. xii + 361 pp. Illus. $5.00. Princeton Univ. Press. Princeton, N. J. 1947.

DO THE films of a nation reflect its mentality in a more direct way than any other artistic medium? Is the motion-picture screen a reflector, a mirror, a recorder of the existing mass desires of the people? Is it true that the technique, the story content, and the evolution of the films of a nation are fully understandable only in relation to the actual psychological pattern of a nation? Will a story of films made in Germany during and after World War I help one understand the ascent and ascendancy of Hitler?

Siegfried Kracauer would reply yes to all these questions. Some interesting hypotheses have been stated by Mr. Kracaucer, but I think that an affirmative answer needs more qualification than he presents.

Here are some of the characteristics of the German mind that Kracauer thinks were reflected in German films during the interwar period:

1. A deep-seated fear of social change and thus a welcoming of films which defamed not only bad rulers but also good revolutionary causes.
2. A strong sadism and an appetite for destruction. The sadism appears to stem from a basic inferiority complex.
3. Life represents a choice between a regime of tyranny or a cataclysm of anarchy. Authority will protect society from decomposition.
4. An inner change of the individual counts more than any change of the outer world—an implication justifying the middle-class fear of social change.

My reader can plainly see the problem involved in presenting such a thesis as Kracauer's. According to this theory, the writer, the director, and his staff are consciously (or unconsciously) reflecting certain values. The public who sees a film consciously or unconsciously is appealed to by these values. But a motion picture does not even need to be popular ("Caligari" is an example) to reflect these desires and longings.

To check on the validity of Kracauer's hypothesis, we might apply it to the films made in the United States today. What mass desires are being appealed to by the five hundred or so motion pictures we produce annually? What does the heavy emphasis on crime and violence in our movies mean? Is our free and easy emphasis on "rubbing out" people, kicking, slugging, shooting, or torturing them a reflection of a mass desire to hurt, to wound, to get even? Simply to ask questions such as these is to suggest the difficulty of answering them.

Kracauer's hypothesis is a highly suggestive one for film analysis even though he did not prove his hypothesis to the satisfaction of this reviewer. A film, it seems to me, is not a mirror of life except in a highly metaphorical sense. It is, rather, life seen through the eye of a writer, then through the eye of a producer, then through the eye of a director, then through the eye of a camera, and then through the eye of a cutter. What usually comes out is a reflection not only of society, but of what the producer thinks will make money. This final product is inevitably related in some degree to the mass desires, wishes, and longings of the movie-going publics in the United States and the rest of the world.

The discovery, however, of the factors finally responsible for what appears in the film is a huge psychological and sociological task, which Kracauer's volume will help to solve. His volume also contains a great deal of useful information about the history of the German film. It is fruitful reading for any serious student of motion pictures.

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COMMENTS AND CRITICISMS

RACIAL INTELLIGENCE

In an article concerning racial intelligence in the October issue of THE SCIENTIFIC MONTHLY, Professor Henry E. Garrett begins with the statement that "the question of the existence of Negro-white differences in mental ability within the United States has of late been sadly confused with social and political issues of racial superiority, discrimination, and the like." This is a very generous admission on the part of Professor Garrett, but perhaps gratuitous, for it would be hard to find a more excessive example of this kind of confusion than his article presents.

Although Professor Garrett claims to be interested in the *facts*, what he deals with primarily are interpretations. The facts are sound enough; the interpretations, I believe, can be shown to be more than questionable.

Let us, then, take Professor Garrett's statements passage by passage and see how they stand up under a critical examination.

"A study of Negro-white differences in the United States offers certain advantages—as well as disadvantages—to the student of racial psychology. A decided advantage is the fact that Negroes and whites have lived side by side in our country for more than three hundred years."

For a native of Virginia (if the biographical account appended to the article is correct) to have written the words "Negroes and whites have lived side by side" betrays either a singular incapacity to perceive the obvious or else a disingenuousness of the most inhuman kind. If there is one thing that Negroes have *not* done anywhere in the United States it has been to live "side by side." And it is the whites who have prevented this by erecting barriers of every possible sort calculated to keep Negro and white apart. When, then, Professor Garrett writes that "the American Negro's native language is English, and he has been exposed to the same manners, customs, and environmental influences (schools, churches, movies, etc.) as the American white," I think he is not only misinterpreting but, what is worse, that he is also misstating the facts. In the land in which Jim Crowism and segregation in "schools, churches, movies," and particularly in "etc." is the rule, to say that the American Negro is exposed to the *same* "manners, customs, and environmental influences," represents a misstatement of the facts so revealing as to beggar description. If the American Negro participates in white culture at all (and that he does so is obvious), it is only more or less peripherally. The Negro's "place" has been on the outskirts of the periphery of American culture. He has rarely been allowed to penetrate beyond. If Professor Garrett is really anxious to know the facts let him read the collection of them made for 1942–43 by Professor Howard W. Odum in *Race and Rumors of Race* (Chapel Hill: Univ. of North Carolina Press, 1943). In this book he will find Professor Odum asking why it was that Southern conduct toward the Negro was so contrary to all principles and preaching; why it was that the tenets of fellowship and Christian religion did not hold with respect to the Negro. He will find Professor Odum (himself a Southerner) returning the considered judgment that the only answer is "that the Negro did not come within the framework of human brotherhood" (p. 23). Let Professor Garrett read Gunnar Myrdal's *The American Dilemma*, a book that presents the facts *dispassionately* analyzed by a Swedish scientist. Possibly these two works might show him that it is completely and, I may add, viciously, untrue to say that the American Negro has been exposed to the same influences as American whites. But does one have to read books to know this? I think not, and I also think that no amount of reading of books or acquaintance with the *facts* is necessarily capable of modifying beliefs that a person *wishes* to believe. But this is a commonplace of psychology.

"We may begin with McGraw's study in 1931 of the comparative abilities shown by 68 white and 60 Negro babies, two to eleven months old, all living in a Southern community. This study is valuable in spite of the small samples because social influences are minimal if not completely absent at these early age levels."

To students of the socialization of the child it will come as a surprise to learn that there is still a psychologist in existence who believes that "social influences are minimal if not completely absent at these early age levels," specifically, from two to eleven months. Most students of the social development of the child would agree that the most fundamental steps in the process of becoming a social human being are taken well within the first year of life; that the socialization of the basic urges or needs of the child commences almost from the moment of birth; and that by the end of the first year different habits of behavior usually associated with different social classes are already easily perceptible. Babies in different cultures are already during the first year perceptibly different in the nature of their responses. Professor Garrett would doubtless put this down to their genes. The anthropologists and the child-study groups, the pediatricians, and most psychologists think differently. Margaret Mead, Bateson, Benedict, Dennis, Erickson, Kluckhohn, Mowrer, Dollard,

Gesell and Ilg, Helen Thompson, Margaret Ribble, Goldfarb, and numerous others find that the first year is perhaps the most important in the whole social developmental life of the person. But not so Professor Garrett.

The developmental quotient of the small sample of Negro infants to which Professor Garrett refers was 92, some 13 points lower than that of the white infants. Differences in the socialization process might very well explain such a finding. In any event, the sample is heavily prejudiced by a difference in socialization for which no allowance whatever is made. On the other hand, such differences in social status as were reported are minimized by Professor Garrett, and the very significant fact that the white infants were taller and heavier than the Negro infants is dismissed as irrelevant. The fact that height and weight are almost universally regarded as the best measures of the developmental status of infants may not be of relevance for Professor Garrett's argument, but so far as the interpretation of the facts is concerned, I submit that it is of some importance. Taller and heavier children would be expected to show a higher developmental quotient.

With respect to the I.Q.s of Negro as compared with white children, I do not think that it is possible to draw any conclusions from the tests as they are at present set up other than that what they measure is socioeconomic experience and schooling. Where these variables have appreciably varied, the results obtained should be expected to vary. And this is, in fact, what we find. Income and per capita expenditure on education is highly correlated with I.Q. score. This has been recently demonstrated by Drs. F. L. Marcuse and M. E. Bitterman, of the Department of Psychology of Cornell University, with respect to the scores obtained on the Alpha and Beta tests administered to draftees during World War I ("Notes on the Results of Army Intelligence Testing in World War I," *Science*, **104**, 1946, 231–32). From their results the authors write that "it is probably most warranted to conclude . . . that the Beta scores, like the Alpha scores, are strongly influenced by cultural factors concomitant with the socioeconomic levels of the states" (p. 231).

In an analysis of "Intelligence Scores of Northern and Southern White and Negro Recruits in 1918" (*J. Abnorm. Soc. Psychol.*, **39**, 1944, 471–74), Professors Thelma G. Alper and Edwin G. Boring, of the Department of Psychology of Harvard University, conclude their findings with the words: "It is the Negro's educational disadvantages and not actually his color which handicap him in these tests" (p. 474).

My own findings are in complete agreement with this verdict ("Intelligence of Northern Negroes and Southern Whites in the First World War," *Amer. J. Psychol.* **58**, 1945, 161–88).

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Dr. Garrett has participated in a number of polemics on the subject of Negro intelligence, one of the most recent being in your sister-journal, *Science* (1944–45). At that time his remarks were adequately refuted by others more competent than myself in the field of racial psychology.

In his latest article, however ("Negro-White Differences in Mental Ability in the United States," SM, October 1947), he bases a large part of his argument upon McGraw's study of Negro and white infants in which she found the former to be significantly inferior. This was until recently the only study of its kind in the United States. Dr. Garrett states incorrectly of that study that ". . . the standard height-weight measurements . . . show the Negro children to be as typical of their age levels as were the white children." Not only were the height-weight measurements below those of national Negro norms (which are far below white norms), but the white female figures were above the norms quoted by Dr. McGraw.

In a paper published last year (*J. Genetic Psych.*, 1946, 69, 3–44), I describe a group of New Haven Negro infants whose development was fully equal to that of the white babies upon which the *Gesell Developmental Schedules* are based. Significantly, the birth weights and physical development are equal to local and national white norms. This study was done during the war years when despite (or perhaps because of) rationing Negro mothers for the first time in American history secured a diet approaching that of white standards. Within the past decade the great importance of prenatal nutrition upon infant welfare has been indicated by a number of workers in the field.

The infants of our study have now been examined for a third time (ages nineteen to thirty-two months) by Dr. Hilda Knobloch and myself (report not yet published), and we find that their development continues at the initial rate.

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MAN, THE MYTH-MAKER

It is not my habit to indulge in *pro bono publico* letters, and I certainly am not the friend of censorship or anything which savors of curbing free speech; but the article on "Man, the Myth-maker" in your issue of July 1947, does provoke the observation that freedom of speech implies a responsibility to use canons of good sportmanship.

In the article referred to, on page 62, the writer is speaking of what he calls "crude anthropomorphic myths" which have recently merely suffered a word change even though utilized by reputable thinkers. Says the writer: "Witness the Pythagorean assertion of a great modern scientist that God is a Mathematician (maybe it was a joke or whimsey) and the feminized Platonic Realism of Mrs. Eddy (certainly not a joke)." The writer continues his apparently amused contempt for the millions who

still believe in such "crude sacred myths" as personal immortality. From that he goes on to pillory "millions of believers in various more or less mythical concepts including fifty-seven other varieties of semi-sacred myths that deny the evidences of the senses and the findings of science."

Permit me to observe for the benefit of this writer, and others of similar temper, that sadism is not science, nor is sarcasm the height of wisdom or the scientific spirit. It apparently never occurred to this writer that the most fundamental principles in physical science must and do contradict and deny the "evidences of the senses." Such contradictions are indeed the very foundations of science. He evidently still labors under the naive idea that physical science, with its apparatus and methodology, has attained firsthand contact with "reality." The genuine scientist knows, and frankly states, that the best he is able to do is to give us a symbolic representation at secondhand of what the universe really is. Indeed, the "findings of science" themselves have not infrequently been myths with emotional response, persecution, invective, and all the other characteristic stigmata of handling the unbeliever. Does this writer really think that scientists like Steinmetz, Carrel, Eddington, Jeans, Dampier, or Lecomte de Noüy are below par mentally because they recognized the reality of psychic and spiritual forces above and beyond mere sense-appearances? I wish we had more of their equals in the field of sociology!

The whole history of mankind insofar as it records progress has consisted in the stripping off of layer after layer of false belief and outworn concepts. We welcome every new advance of truth which enables us to discard the old beliefs however precious they may have seemed tc be. At the same time, scientific progress is never made by rudeness or overstatement or deliberate affront. The appeal that convinces men is not oratorical, harsh, but the winsome, straightforward, humble, and modest presentation of fact. The gates of the kingdom of science and of truth are never opened by name-calling or *argumentum ad hominem.*

ARTHUR J. TODD

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I am writing to congratulate you on publishing Read Bain's article on "Man, the Myth-Maker" in the July number of SM. The article is very timely and highly important. If we are entering a new scientific era, we must put off the old traditional myths we have been brought up on and make all things new. Myths developed during man's most ignorant and degraded period, namely, the half million years or so during which he was slowly rising from the animal level to the somewhat higher level he is now on. As we enter the new era of knowledge and understanding we must free ourselves more and more from these old myths. This is almost as important for the scientist as for the layman. Have you read the article by Dingle in *Nature* (London) for July 26, page 108? He points out some of the shortcomings of even our top scientists and contends that the greatest lack and corresponding need of science is for a constructive self-criticism. He attributes this lack to the fact that scientists are digging industriously, each in his own little dark corner of the great unknown, and seldom come out to look around (in Bain's terms, to scrape off some of the myths).

It may be the most important function of the SM to expound this new point of view and to prepare our people for the new world we are now entering.

An incidental reason for this letter is to help counterbalance the letters you may receive against having published Bain's article.

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BRIDGMAN AND COHEN

In the August 1947 SM the semitechnical articles by Janssen, on rock-weathering, Dellinger, on the ionosphere, and Lewis, on the distribution of wealth in a Mexican village, are all very interesting. The last of these may be of some value as indicating the natural equilibrium of wealth distribution in communities, a distribution the New Deal has been trying to upset by stultifying the efforts of the able as far as possible, unless they were politicians or in the good graces of the politicians.

The articles by Bridgman and by Cohen are likewise interesting, but for a totally different reason. Whereas the three articles above-mentioned enable us to enjoy the great outdoors in a detached fashion, these last two are highly charged with personal feeling. Cohen, and the collectivists who think like him, are a threat to scientific detachment, which Bridgman, and those who think like him, regard as essential to a scientific frame of mind.

I did not notice, at first, that Cohen was a lawyer, according to the small type under the caption, but I became aware as I read on that his work was a piece of special pleading, a lawyer using his brilliance, rather than his conviction, to plead a bad case; or the performance of a magician, dazzling the audience with a convincing demonstration of things that we know perfectly well are not so.

Bridgman's article gives the impression of sincerity, a rather simple sincerity, though in some of his scientific writings I have suspected him of taking a Puckish pleasure in teasing his readers with conundrums. But here and there is a note, not insincere, of bitterness and alarm, as when he defines the "present philosophy of the relation of the individual to society" as the "right of the stupid to exploit the bright."

The bitterness and alarm are justified, but the phenomenon is not quite as stated. The "stupid," even if they should turn out to be more numerous than the "bright," are incapable of exploiting them. They are too stupid to do so, even if they wished; and being stupid does not necessarily involve a

wish to exploit others. The people of less than average intelligence are often simple, sincere, and reasonable, though they may be sometimes a little tiresome to the "bright."

The real trouble is that the "stupid" are egged on by those who think, like Cohen, judging from his reference #9, that the preferential exploitation of the bright, and of the successful or wealthy, is desirable. The trouble is not with the stupid, but with the bright demagogues who exploit their stupidity and, through their votes, hope to exploit the bright ones who mind their own business.

As a matter of fact, the dissociation of the scientific investigator from worries as to how his discoveries may be misexploited by lawyer and politician is important to his success as a discoverer. To this extent Bridgman is absolutely right. Therefore, as a matter of ethics, society has no right to place upon him a burden that will destroy him as a scientist. But, as a matter of fact also, the scientist has something the politican wants: he has knowledge, which is power. The politician wants that power, power over other men, which that knowledge represents: to him this power is wealth. He does not possess the ability to create the wealth, so he must create a situation in which he is dominant over the scientist. This is where the votes of the "stupid" come to his aid. He can create the impression that the stupid are exploiting the bright, but it is really the power-hungry "bright" ones, who understand the "stupids," exploiting those "bright" ones who are not power-hungry and care for the knowledge that is power, rather than for power itself.

Psychologically, I suppose the most characteristic thing about scientists is that they have no ambition to lord it over other men. Some are sociable, and some are shy: some are easy to get along with, and some are definitely cantankerous. Some make good executives, as was strikingly demonstrated in the OPM and WPB, but very few really want to be executives, and a large proportion of them ask little of other men, except that they keep out of their way.

This is where the danger lies. They have power, which is untold wealth to another type of mind, and they have no use for it. What a group to plunder! It is like taking candy from children.

But here enters another phenomenon, utterly perplexing to the politician. The scientists are perfectly willing to give their knowledge to all the world: they do so in "scientific papers." The politician would rather have the knowledge donated to himself and kept from others, for that would increase his power. The original Kilgore Bill seems to have had this viewpoint.

But a still more exasperating situation follows. When the scientists have turned over to the politicians their knowledge, their wealth of power, it evaporates in their hands. It ceases to be power, because the knowledge, which is freely given, is beyond the ability of the politician to accept. Present him with all the scientific papers that were ever written, and kill off all the scientists, and he has nothing but waste paper. Rome in the hands of the Vandals is no longer Rome, save in name.

Hence arises the demand for control of the scientist. It is not the power of the scientist that must be snatched from him: it is not the knowledge of the scientist that must be exploited: it is the scientist himself.

The situation is bad; but desperate situations often produce great men to handle them. Although the mind of the typical scientist and of the typical politician are far apart, it is not inconceivable that from among the ranks of scientists there may arise a consummate statesman. If so, though he will in the end be sacrificed to the demagogues and the mob, he may be able to accomplish something of the synthesis that the arguments of both Bridgman and Cohen demand.

As things stand, the dilemma continues. The scientist wants to be left alone to be a scientist: the politician wants him chained to his chariot on his triumphal processions. He wants to be able to show the mob how marvelous are the results when the politician makes the scientist do his bidding, and what a benefactor of the nation the politician is when the scientist is compelled to keep in mind continually the social possibilities of his discoveries. This demonstration will be false, but it will be effective if we are not alert and careful.

The politician, in trying to throw the odium of the misuse of science upon the scientist, is confessing his own incompetence and dodging his own responsibilities. But to the extent that the politician is incompetent, the scientist must help. If we have some statesmen with even a modicum of competence, and of a fair standard of honesty, it should not be difficult for at least some scientists to collaborate with them, giving them help and advice from time to time, without feeling exploited or exasperated at the loss of their time from research proper. This is the only intelligent compromise. But it should not be forgotten that power is no concern of the scientist, and that the statesmen must accept the sole responsibility for its exercise. Its misuse, its unintelligent use, will become increasingly dangerous as science progresses. A higher standard of statemanship will be demanded. Less preoccupation with the exploitation of the votes of the "stupid," the uninformed, and misinformed will be demanded, even if it means that our present crude concept of "democracy" as mob rule by an uninformed majority has to be amended.

At present the scientist is afraid of the politician: he may well be, after the violent and illiberal and dishonest career of the New Deal. The politician is also afraid of the scientist: he may well be, after the atomic bomb and the supersonic rocket. But not all statesmen are knaves, and not all scientific discoveries are harmful.

The scientist may regret, or even resent, every moment's diversion from his researches: he may be convinced, and rightly convinced, that every such diversion is a net loss. But we have to be practical. The injunction, addressed to men of otherwordly ambition, still stands, to be as wise as ser-

pents and not simply as harmless as doves. And wisdom consists in recognizing what has happened to scientists, what power has been committed into their hands, what dominion over all the earth, and that the fear of them is upon every living creature.

Into their hands the future of the nation and of the world is committed. With that power necessarily goes responsibility. Great power was for a decade or more granted to the trade unions, but responsibility they refused. Are scientists going to imitate them? The Congress and its committees sought the advice and suggestions of the unions, to frame an equitable law, and met with no co-operation whatever: now the unions are howling about a slave-labor act. A measure of collaboration from scientists is essential, even at the loss of some of the time we should like to devote to research; otherwise we also shall soon be complaining about the enslavement of the scientist, and it will be more than an exploitation of the bright by the stupid—and far worse.

The detestation in which we hold some lawyers, some politicians, some legislators, their stupidity and their cupidity, the shrewdness that substitutes for intelligence with some of them, must not prevent our recognizing that this country is entitled to good government, the best government we can provide, and the best will not be provided without the help, very substantial help, from scientists. Government by statesmen advised by scientists would appear to me to come near to being ideal, on an intellectual basis, provided that fundamental honesty (on which scientists are long) and on decent respect for the limitations of human nature (on which many scientists are short) are also part of the set-up.

We do not want another doctrinaire brain-trust; we need wisdom as well as "brightness." And let us hope that scientists can show such a degree of human sympathy and understanding that we do not live to see them discredited and detested like the irresponsible leaders of the trade unions.

Statesmen must not demand the souls of scientists, nor too much of their time. That is not the way to make a great nation. But they are entitled to the support of scientists, to their advice, to some at least of their time. Scientists cannot be excused this contribution, as a matter of practical politics, any more than they can be excused their share of taxes. Nor should they ask it. They are entitled to press vigorously for a reduction of everyone's taxes, for a change in that vicious system (which Cohen clearly approves) of demanding from each, not according to his ability, but according to the square or cube or some higher power of it; and they are entitled to see that scientists shall not be put under excessive contributions of their time, merely because they are scientists.

Bridgman is right in insisting that the responsibility for the social or unsocial use of science is a responsibility that lies upon the whole society. Cohen is not right in insisting (as I think he does) that a special part of it lies upon the scientists. The use made of science is not a scientific phenomenon, it is a social one. All the citizens share the responsibility. The scientist takes his share only because he is a citizen; and scientists as a whole take a very minor share, because they are few. The crime of misapplying science is squarely on the shoulders of the great majority who are not scientists. The sin of omission in not applying every scientific discovery to some socially desirable end also rests upon society as a whole. Scientists are not now, and never will be, numerous enough or clever enough to see that no useful application goes unexploited, no unsocial one ever gets practiced. They are not policemen: they are not mind-readers: they are not magicians: they are scientists—seekers after truth.

Somewhere close to Bridgman, and a long way from Cohen, is the ideal society; for science cannot flourish in Cohen's society, and society cannot flourish without science.

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EINSTEIN'S ERROR?

The following criticism is made on the assumption that Dr. Gould ("The Theory of Relativity and the Atomic Bomb," SM, July 1947) has given an accurate exposition of the relativity theory. It appears to agree with several such outlines that have been studied over the past thirty years and, if anything, appears to be the most lucid of those in recollection. For this reason, it seems also to be the most suitable one as basis for the following criticism. It seems, in fact, to reduce the theory to absurdity, although there is obviously no such intention on Gould's part.

Professor Einstein's theory of relativity has, of course, been questioned before, and, particularly recently, some of his derived formulas have been under concerted attack by physicists and engineers because the same results can be obtained by different, and seemingly more logical, formulas. The purpose of the present discussion is to show that the original, special relativity theory, which he published in 1905, not only assumes conditions that do not exist, but in addition uses a mistaken theory of light to explain the hypothesis. It is supposedly based on a scientific dilemma arising from experiments carried out by Fizeau in 1851 and Michelson in 1881, which are said to disagree with Newton's "principle of additive speeds" (1687).

Fizeau is supposed to have found that, when light was passed through a liquid in rapid flow, "some but not all of the velocity of the liquid was imparted to the light" (quoted from Gould) as compared to passage through the motionless liquid. In seeming contrast to this, Michelson found that when he reflected a beam of light in the direction of the earth's motion (18 miles per second) and another beam for the same distance at right angle to the first, both beams made the return trip in exactly the same time.

These experiments embody several dissimilar elements, for which apparently no comparative data exist, such as different direction substituted for parallel direction in quiescent and flowing medium, and gaseous for liquid medium. Even so, there is probably no mechanism by which Newton's corpuscular theory of light could account entirely for these findings, but the undulatory theory of light does so quite readily and, since modern views tend more in this direction, there is no further cause for assuming that any dilemma is presented by these experiments.

Gould's article employs Einstein's "moving-train-and-platform" simile to explain his hypothesis. This is presumably copied almost verbatim from Einstein. It is stated that the velocity of light through air or vacuum is so great that "nothing whatever can be added" to it, and only part of any superimposed velocity can be added to the lesser speeds of light through various liquids, etc. These conclusions are supposedly drawn from the above-mentioned experiments and from other similar evidence.

The constancy of the speed of light in air or in vacuum is said to abrogate Newton's principle of additive velocities and is supposed to be due to its enormous magnitude, whereas, since all known facts point to a different mechanism of light propagation than Newton assumed, it is the theory of light that is at fault. It is not a question of magnitude, but one of the underlying phenomenon involved in light propagation.

The velocities of undulatory or wave motions are dependent only on the characteristics of the medium in which they travel. In the case of light, the possibility of superimposition of two media must be assumed. Light cannot properly be said to consist alone of vibration of the bodies through which it passes, as sound is entirely accounted for by vibration of the air, liquid, or solid through which it passes. Unlike sound, whose velocity increases in accordance with the elastic properties of the media and in general with their density, the velocity of light is markedly reduced in accordance with the optical density of bodies located in its path.

Light must be dependent for its speed on a combination of the properties of at least two media when traveling within the earth's atmosphere. The situation is somewhat like that of a stream of water permeating, or running through, a log jam, which may also travel at its own speed, dependent on friction against the shore, although the effect of the logs themselves on the speed of sound in this water would in this case be negligible. Their effect would be limited to a slowing up of the stream with attendant slowing of the sound. In the same way, light, passing through transparent bodies, is always impeded, but more or less so depending on the state of motion of these bodies. In no sense could their entire velocity be added to that of light.

Regarding any effect of the motion of a light source on the velocity of light, the Doppler principle (1842) states explicitly for sound and light that such a motion affects the frequency or wave length—*pitch* in the case of *sound*, and *color* in the case of *light*. This principle is amply substantiated by experience, and it is also well known that frequency and wave length are unrelated to the speed of transmission. Were it not so, music would become distorted (refracted) when heard from a considerable distance or when transmitted by FM, because the higher notes would travel faster than the lower ones. It would also presumably be impossible to be hit by a projectile before hearing it. It is also known that all electromagnetic waves travel with the same velocity, even though the frequencies of known waves vary widely.

The entire train-and-platform simile, purporting to show that there is an apparent infinitesimal difference between the time on the train and that on the platform, therefore becomes meaningless. The assumption of a real difference, leading to an interrelationship of space and time, would have to be based upon a superimposition of a timeworn theory of light (Newton's) on an erroneously assumed effect of the speed of a light source on its velocity, and certainly makes a very artificial and questionable basis on which to found the general theory of relativity (1915).

As stated above, any distinction between the behavior or speed of light and Newton's underlying principle of additional speeds is not based on magnitude, but on an understanding of the type of phenomenon involved. Newton's principle obviously does not apply to wave motions in the form used by Einstein. As far as the formula $E = mc^2$ is concerned, would it not be almost as correct and informative to say that $E =$ *much, much, much* since we are as yet very far from being able to ascertain how much *more* energy the atom might contain than that expressed by this formula? Certainly, the fact that the speed of light appears to be a universal constant must be based on entirely different reasoning than this.

Exactly what Fizeau thought he discovered is in doubt without extensive research. It does not seem possible that he or anyone else (except perhaps a mathematician gone rampant) could have thought that any of the velocity of a liquid could be added to that of light passing through it by any type of mechanism, corpuscular or undulatory, unless the liquid travels at a greater speed than the light. At any speed below this the liquid must (and does) slow the speed of light, as indicated by the refractive indices. It would be very interesting to make a detailed analysis of his results in accordance with the ideas of Fresnel (1788–1827).

The fact that liquid flowing in the same direction as light slows its velocity less than motionless liquid is simply explained by the fact that it has less liquid to traverse. The difference is proportional to the distance the liquid travels during the time required for the transit of light; or, to put it another way, the apparent optical density of the liquid is correspondingly less.

Such investigations might add considerably to the theory of light, but none of this information could possibly lead to an interrelationship of space and time as long as, in Dr. Gould's words, "we can make a transformation of the one time and place into the other." Since, therefore, no uncertainty exists, it is merely a matter of applying corrections in known and time-honored fashion. The fact that all celestial bodies are in motion relative to one another changes this circumstance in no fundamental. A reference-point-at-rest can always be assumed, and it might actually someday be discovered.

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OBFUSCATING PERSONALITY

The August SM and the current number of the *Journal of Clinical Psychology* contrast a bit too sharply on the subject of personality diagnosis. SM carries an excessively enthusiastic piece by Louis Wekstein on projective techniques for "X-Raying the Personality," particularly by use of the Rorschach Test. (The test consists of ambiguous ink blots, which are interpreted, and then explained, by the subjects, clients, patients.)

In the *Journal,* the most relevant publication in this country, edited at the Medical College of the University of Vermont by Frederick C. Thorne, psychologist and psychiatrist, I have occasion in a piece on the psychotherapeutic use of logic to mention with satisfaction the passing "of the vogue of ink spots;" and the editor comments with some alarm on the "opportunities for the growth of quackery which currently exist in clinical psychology particularly in the fields of diagnostic testing and counseling. The development of projective methods . . . has opened a fertile field for those who purport to be specialists. . . . Riding on the band wagon of the current popularity of the Rorschach test, these self-appointed experts solemnly give ponderous opinions on personality problems which seem ridiculous to more sophisticated observers."

I regret need for caustic comment on personality X-raying, for I am a devoted reader of SM, but the plain truth of the matter, somewhat overdue, is that the Rorschach test during its decade of popularity has not been proved reliable or significantly valid for useful diagnosis in the opinion of the majority of psychometrists. Much evidence has appeared during the past year, particularly in the *Journal of Applied Psychology* and in *Educational and Psychological Measurements.* On the other hand, by the test the schizoid have been "proved" schizoid, the neurotic have been found neurotic, and the Nazi war criminals have been discovered somewhat paranoid after months of imprisonment. The ink blots are sufficiently phallic to prove entertaining, and the test is clearly superior to extrasensory perception, but it is my observation that clinical psychologists and psychiatrists who use it usually shift from one system of scoring to another and finally administer the test mainly for window-dressing purposes ("scoring" by the inspection method), or else they shift to the use of dominoes. One needs only give an ink spotter his honest "protocol" as if from his maiden aunt and then ponder the personality description obtained.

Hasty popular enthusiasm over cure-all psychotherapies is pitiful and, in our culture, probably excusable; persistent professional promotion of a catch-all scheme for diagnosis may be pitiful, but is hardly excusable in any culture. Handwriting analysis, anthropometric measurement, hormone assays, dream interpretation, free association, picture interpretation, description of ambiguous figures, conversational reaction time and dominance, facial responses (not features any more), polygraphic records of subliminal responses, and so on and on are all significant for the understanding of personality, in a research way, but they are not promising in clinical diagnosis for the selection of therapy unless (*a*) one has infinite time (and the client has lots of money); (*b*) one is dealing with a pathological liar; (*c*) one does not know of, or cannot think of, very many direct and appropriate questions; and/or (*d*) one puts together many clues from such sources. Indeed, the current problem in clinical diagnosis is to detect many clues and to put them together into the most significant pattern rather than to discover one curtain to draw or one button to press.

Experimental evidence in quantitive terms is as imperative in psychology as in any other science and, unfortunately, more elusive. Scientific colleagues in other fields do not help us by proving credulous in ours.

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THE SEA'S PHANTOM BOTTOM

The echo-sounding layer described by Dr. Chapman in the March SCIENTIFIC MONTHLY was discovered off the coast of California by scientists of the Navy and University of California. It has since been observed elsewhere in the Pacific, and in the Atlantic. Dr. Robert S. Dietz, who accompanied the Navy 1947 Antarctic Expedition observed it regularly in the daytime as far south in the Pacific as the Antarctic Convergence.

Originally described as the "deep scattering layer," the phenomenon was recently rechristened the "phantom bottom" by the Hydrographic Office, owing to the erroneous reports of shoals that have resulted from misinterpreted fathograms. A significant feature of the phantom bottom is that it is observable only in the daytime. Within a half hour after sunrise the layer, if present, is recognizable at depths of 125–375 fathoms, and it vanishes after sunset in as short a time. It disappears at night, presumably because whatever is causing it has moved up so near the surface that the return echoes are merged with the outgoing "ping" of the sound device.

The depth at which the phantom bottom occurs is too great for it to be associated with photosynthesis, and the diurnal variation rules out any other physical or chemical process in the sea as the cause. Of biological agents, only zooplankton or nekton can be responsible.

An organism moving vertically 200 fathoms in a half hour is making 40 feet per minute. While zooplankton are known to migrate vertically, it is doubtful whether any of the common forms are capable of such speeds, and Dr. Dietz moreover doubts that they are large enough to scatter sound of fathometer frequencies. As for fish, neither fishermen nor ichthyologists are aware of any forms occurring in abundance sufficient to account for echoes from the equator to polar waters. Furthermore, although fish are capable of swimming at speeds much faster than 40 feet per minute, it is questionable whether they actually do migrate vertically at such rates near the top layers of the sea over distances of several hundred fathoms, owing to the effect of the large changes in hydrostatic pressure on their delicately adjusted swim bladders.

Besides fish, the only other widely distributed component of nekton is the cephalopoda. Squid do not have swim bladders, and their morphology and mode of propulsion render them practically indifferent to hydrostatic pressure. They are known to migrate vertically, rising to the surface at night. Are they present in sufficient numbers to provide echoes over most of the waters of the earth? Judging by the distribution of their predators, they are. Cuttlefish form the sole food of sperm whales, which are encountered, well-fed and fat, everywhere from high northern latitudes to the Antarctic Convergence. One would expect sperm whales to require a concentrated food supply of the order of magnitude of that of their cousins, the whalebone whales, which thrive only in the regions of richest production of zooplankton.

Squid are likewise the sole food of bottle-nosed whales, which are abundant enough to form the object of fisheries in various parts of the world. Squid are the chief food of all the other toothed Cetacea, except killer whales; they are a major portion of the food of the fur seals both of the Bering Sea and of antarctic waters; and they are a large portion of the food of many sea birds. All these warm-blooded animals require proportionately more food than comparable fish or invertebrates, and their abundance implies the existence of a vast cephalopod population.

It is therefore suggested that the "phantom bottom" is made up of the echoes from a concentration of squid, hovering below the illuminated zone of the sea and awaiting the arrival of darkness in which to resume their raids into the plankton-rich surface layers.

JOHN LYMAN

Washington, D. C.

CRUMBLING ROCKS

Dr. Raymond E. Janssen's article, "Crumbling Rocks," in the August SM is a very instructive survey of weathering and erosion, but he makes some statements as matters of fact which might be questioned. He says: ". . . the surface of our moon . . . [where] weathering and erosion do not occur. Because there is no atmosphere and no water on the moon, its surface remains unchanged except for the impact of falling meteors."

Apparently, talus slopes occur on the moon where the temperature changes in a short time from about 250° F. above zero to possibly 150° F. below. Such a change in temperature must disrupt the rock to some extent and be classed as weathering. The falling fragments chip the mountainsides as they slide and roll down. Their effect may be slight, owing to lesser gravity and kinetic energy, but such abrasion as they make is true erosion. There are advocates of the theory that a lunar atmosphere exists. Finally, a "meteor" is the light caused by the ablation of the surface of a meteorite. It is the meteorite that causes the impact.

Dr. Janssen continues: "The profound effects resulting from the combined action of weathering and erosion account for nearly all the geologic changes that occur on the surface of the earth."

The effects are profound, but I believe that they are all the direct or indirect result of impacts of huge meteorites, of which the badly eroded and jumbled crater rims would be more easily interpreted from the moon. Certainly the moon has not had a monopoly on meteorites.

CHAPMAN GRANT

San Diego, California

I have just finished reading the article "Crumbling Rocks," by Professor Janssen, in the August number of SM. It is well written and illustrated but hardly a step in the advancement of science.

I have no quarrel with any statement made in this article. It is all true, but "old stuff" that has been well known for at least fifty years and can be copied from almost any geology textbook. It *is* illustrated with a few new photographs.

It reminds me of what *Time* magazine used to call "Brisbanalities," referring to Arthur Brisbane's talent for telling the public what they already knew.

Yes, Professor Janssen is dead right! Water runs downhill! Rocks disintegrate! Mountains are worn down to plains! Even Isaiah knew these facts, as the Professor points out in his text: "For the mountains shall depart, and the hills be removed."—Isa. 54:10.

He might have used also that other biblical quotation: "There is nothing new under the sun;" and added, "or in this article."

ALLAN O. KELLY

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TECHNOLOGICAL NOTES

Photonotes. Because of this issue's attention to photography, releases in that field receive attention. News of this kind gets around rapidly among camera devotees, but some of the items may be novel.

"Needle-sharp" pictures, if not actually guaranteed, are at least made possible by the "Measure-rite" range finder of the Brownie Manufacturing Co. The claim that this instrument "accurately measures all distances from $2\frac{1}{2}$ feet to infinity" has special interest; I shall use it the next time I need to measure infinity.

A light for the microscope and for photography, both micro and macro, said to be "radically different," is made by Fish-Schurman Corporation. It appears to be an arc sealed in a tube of inert gas and almost pinpoint in sharpness—the bright spot is less than a millimeter in diameter.

A camera especially designed for recording oscilloscope traces would appear to have a good deal of usefulness. Fairchild's "Oscillo-Record Camera," said to be the first for such specific purpose, will take still pictures or catch the traces on film moving from 1 inch to 3,600 inches per minute.

Color photographs of the reflections of tiny particles, as revealed by the ultramicroscope, may be better in furnishing information on some substances, such as plastics and rubbers, than the higher magnifications possible with the electron microscope, according to Professor Ernst Hauser, of M.I.T., before an American Chemical Society section. The technique to use, of course, depends on the material and the purpose. Some delicate substances may be "burned" by the penetration of a stream of electrons.

Professional-size pictures are generally $8'' \times 10''$, the Eastman Company tells us, but the improvements in equipment make it possible to get the necessary detail and accuracy in smaller size, so Kodak's new view camera takes them $4'' \times 5''$. The quarter-size negative is economical and still can be enlarged and touched up professionally.

Still other Eastman offerings: photographic papers so sensitive and so contrasty that in reproducing business documents clear, sharp prints may be made from worn and faded drawings, and copies may be more legible than originals.

Amateurs can select the proper combination of f-stops and shutter speeds easily with the aid of General Electric's exposure meter Type PR-1. The announcement interests, but doesn't enlighten, by saying that the meter "remembers" conditions and "reminds" the photographer to check the range of light on the subject.

Announcement of 16-mm. movie-camera lenses, fluoride coated to cut down reflection, is a reminder of the practical extent of this recent development. A 30 percent increase in light transmission is higher than I would have expected, but that's what Bausch & Lomb says it is.

Where there is exposure to rays from radioactive material, employers and workers can be put at ease by a service of Tracerlab. A "badge" of film, fresh every week and worn over the heart (as the picture of a pretty girl in the release shows), is developed and noted. Anyone getting excessive doses of radiation can be detected and protected in time.

Stereographic photography with satisfying solidness, according to an article in Bausch & Lomb's periodical, *Educational Focus,* is the "Trivision" method attributed to a Mr. Douglas F. Winnek. It is stated to be much more than a fad, and promising for future development. Instead of being a mingling of views taken with two cameras, this picture is caught in a whopping B & L lens—looking like a locomotive headlight, the account says—and separated in the film, which is faced with tiny plastic buttons, 300 of them to the inch. When a transparency of such a film is viewed, one eye sees part of the picture coming through the buttons—each a tiny lens—and the other sees a composite of slightly shifted images. No stereoscope is necessary.

M. W.

THE BROWNSTONE TOWER

The American Association for the Advancement of Science assumes no responsibility for the statements and opinions advanced by contributors to its publications. Views expressed in editorials are those of the editors and do not necessarily represent the official position of the American Association for the Advancement of Science.

The foregoing statement appears for the first time under the table of contents of this issue and will be repeated in each succeeding issue of the SM. To a thoughtful person it must seem unnecessary to publish such an obvious fact. By its title the A.A.A.S. is committed to the advancement of science. But how science can best be advanced is a matter of opinion that varies among our members. These various opinions are expressed in the SM by its contributors and editor. A published opinion may or may not coincide with majority opinion of the readers on a particular issue. Even if majority opinion on each issue could be determined, it would certainly be unwise to restrict expression of opinion to the views of the majority. Nevertheless, an occasional reader, deeply offended by the views of a contributor, regards his obnoxious ideas as having the tacit approval of the A.A.A.S. and blames the Association, and particularly the editor, for having admitted such "dangerous nonsense" into an Association journal. Therefore the statement quoted above, similar to those in other national magazines, is being added to the SM.

It would be easy to eliminate all contributions to the SM that might irritate some readers. All articles touching on religious, racial, or political questions might be rejected and the contents of the SM restricted largely to the natural sciences. Then no one would clamor for the editor's scalp, but the SM, I think, would gradually and quietly become moribund instead of increasing in circulation. The editor of a general magazine must consider reader interest first, and our readers are certainly most interested in controversial social questions touching science. Both praise and blame following the publication of such articles indicate interest in them.

Many of the unsolicited manuscripts that are sent to the editor are essays presenting the views of their writers on controversial questions. As a rule such manuscripts are not submitted to reviewers because it is immaterial whether the reviewer agrees or disagrees with the opinions expressed. The editor publishes only a few of them and is guided in his selection by the following considerations: The manuscript must hold his attention throughout, it must have literary merit, and must deal with an important subject in a novel way. It should be written by a person of reputation in his field. It is possible that the editor's choice is influenced somewhat by his own views, but it is a fact that he has published many essays containing opinions with which he does not agree. The section of the SM called Comments and Criticisms provides opportunity for readers to point out errors of fact that have escaped the editor or to express their own dissenting opinions. As many critical letters are published as space permits. Thus any bias of the editor can be counteracted and all points of view brought out.

It is unfortunate that at least two months must elapse between the publication of a controversial essay and that of letters pertaining to it. Our critics can help us by writing as promptly as possible after an issue appears. Comments on a particular article or articles should be written for Comments and Criticisms and not for publication as a principal article. If a contributor wishes to write a long essay presenting views opposite to those of a previously published article, he should do so without belaboring the previous article.

Comments and Criticisms in the present issue should be of unusual interest. Two articles that the editor thought would draw fire have done so. These and other opinions are expressed vigorously and at length. New facts and interpretations are presented, and there are more to come. The SM lives and grows.

F. L. CAMPBELL